ROORKEE

HYDRAULIC EXPERIMENTS.

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PREFACE.

This Work is an account of Experiments on Flow of Water begun in Decr. 1874, and closed in March 1879, thus lasting—after deducting various intermissions—about four years.

To make the Experiments intelligible, and afford the means of criticising them, it has been necessary to give the descriptions of the mode of Experiment in great detail. This is simply unavoidable in all physical Experiments. It is this chiefly which has rendered necessary so large a Work. To make it readable, however, by the general reader who may not care for the full details of the Experiments, a short Preface is attached to each Chapter, setting forth the principal Articles containing the gist of the Chapter without experimental or argumentative detail.

A short Epitome of the Chief Results is also given in Chap. I, Art. 6a,b, from which the scope of the Work can be seen at a glance; and a very full Summary of the Results is given in Chap. XXVI, from which the Results can be gathered in fair detail without searching through the Text. A Table of Contents of Heads and Sub-heads of Articles is also given to facilitate reference.

Every possible pains has been taken throughout, first to make the Experiments themselves trustworthy, and next to ensure correctness in the arithmetical computation resulting, (*see* Ch. IV, 32, 33, & V, 25.)

Wherever the published Data, and Results proceeding from them, appeared in any way doubtful, this is indicated by a query (?) attached. Attention has been freely drawn also to such defects

in the arrangements as became apparent in the course of the discussion.

To render the Work tolerably handy, it has been printed entirely in ordinary 8vo. size. Many of the Tables and Plates thus cover two pages each. In a few cases only the Plates exceed this size, thus requiring an extra fold to reduce them to 8vo. size.

Great credit is due to the Thomason C. E. College Press (at which the Work has been published) for the way in which it has been got up. It will be understood that the publication of so large a Work, with such complex Tables (Vol. II) and Plates (Vol. III), was a very heavy* undertaking to an "up-country" Press in India (*see* Ch. III, 8).

A. C.

N.B.—The Author will be glad to receive any criticisms, or notes of mistakes or Errata. Address to the Thomason C. E. College, Roorkee, N. W. P., India.

* covering about 18 months in all.

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CHAPTER-SUBJECTS.

Note.—This Table of Chapter-Subjects is followed by a full Table of Contents of Heads and Sub-Heads of Articles. It is hoped that the latter will serve most purposes of an Index.

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PART IV.—SUBSIDIARY WORK AND CONCLUSION.

CONTENTS.

HEADS AND SUB-HEADS OF ARTICLES.

Note.—The numbers given are those of the Articles which are numbered throughout the Text in black letter (as **14**) on left hand of page; each Chapter has a separate numbering of Articles.

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ABBREVIATIONS.

The following is a List of the chief abbreviations used in this Work. Abbreviations used only in the Tables will be found in Vol. II, Introduction, Art. 9.

ABBREVIATION.	FULL EQUIVALENT.	ABBREVIATION.	FULL EQUIVALENT.
Abstr.	Abstract.	Inst.	Institute.
Approx.	Approximate, Approximation.	L.	Left.
Aq., or Aqued.	Aqueduct.	Maga.	Magazine.
Art.	Article.	max.	maximum.
b.	by, (as in N b E, S b W, &c.)	Miss.	Mississippi.
c. or cub.	cubic.	N.	North.
Ch., or Chap.	Chapter.	N. W. P.	North-West Provinces.
Civ.	Civil.	Phil.	Philosophical.
Co-efft.	Co-efficient.	Pl.	Plate.
Commn.	Commission.	Proc.	Proceedings.
Comp.	Comparison.	Prof.	Professional.
Det.	Detailed.	P. W. D.	Public Works Department.
Disch.	Discharge.	R.	Right.
E.	East.	S.	South.
Eng., or Engr.	Engineers.	Ser.	Series.
Engng.	Engineering.	Supfl.	Superficial.
Exptl.	Experimental.	Surf.	Surface.
Expts.	Experiments.	Tab.	Table.
ft.	feet.	Trans.	Transactions.
hyd.	hydraulic.	Varn.	Variation.
in.	inches.	Velocy. or Vely.	Velocity.
Ind.	Indian.	W.	West.

PART I.

CHAPTER I.

INTRODUCTION.

1. Importance of Discharge-Measurement.—The most important quantity requiring measurement or calculation in practical Hydraulics is the DISCHARGE passing through a channel: and in artificial channels, such as Canals, it is desirable that this should be measurable or calculable with some accuracy.

Discharge-measurements of large Canals and Rivers are being constantly made; but can any reliance be placed on the numerical Results?

Most of the formulæ at present in use involve numerical "coefficients" whose values have as yet been determined solely from Experiments on the very small scale, *e. g.*, from pipes and small artificial channels: the propriety of their application on the large scale seems doubtful, and at any rate requires proper testing.

2. Modern Experiment.—Much systematic and extensive Experiment has been done of late years on Channels of very different kind—

1°. *Pipes and Small Open Channels*, by Messrs. Darcy and Bazin in France.
2°. *Medium Rivers, e. g.*, The Lake Rivers, and Connecticut River in America; and the Rhine, and Elbe in Europe.
3°. *Great Rivers, e. g.*, The Mississippi, La Plata, &c., and Irrawaddi.

The small scale French Experiments (No. 1°) were done with a precision attainable only on the small scale: they are believed to be ample as regards flow of *small bodies* of water, (none of their channels having exceeded $6\frac{1}{2}'$ width.)

River Experiments, though they unquestionably lead to Results of great practical use, are not well suited for attaining to the discovery of *general laws* of fluid motion: as, from a variety of causes, they can seldom be executed with the necessary precision; the inevitable irregularity

of banks and bed leads to a complexity of motion which cannot as yet be unravelled, and traced to its separate causes. The Great River Experiments especially are on motion of too tumultuous a character to admit of resolution.

The River Experiments clearly show that Results depending solely on Experiments on the very small scale are not safely applicable to large bodies of water.

[The author has been informed* that a single Flood Discharge-Measurement of the Ganges at Hardwár varied from 100,000 to 200,000 c. ft. per sec. *according to the formula* used in computing it].

3. New Experiment wanted.—The sort of Experiment now wanted appears to be something on a larger scale than the small scale French Experiments, and yet under simple conditions, viz.—

"Experiment on Large Bodies of Water in Uniform Motion in Regular Channels" executed with a precision approaching that attainable on the small scale. Such conditions are ordinarily attainable *only on Canals.*

4. Opportunities in India.—The presence of many Large Canals points to India as affording unusually favorable opportunities for prosecuting such Experiments *with a fair expectation of a practically useful Result.*

[Experiments on a limited scale have been carried out by many of the regular Canal Staff; but partly from want of special funds, and partly from want of leisure, these Experiments of individuals have necessarily been of a desultory character, and limited in scope: moreover, not having been published, they are practically useless to science.

Experiments on a considerable scale were projected in 1866-67 to be performed on the Ganges Canal at or near Roorkee; but the officers† connected with them all died before even the experimental difficulties in the use of the Instruments intended to be used were‡ surmounted].

5. Objects of this Work.—The primary objects of this Work were then—

i. Discovery of a good Method of Discharge-Measurement,
ii. Testing the applicability of known Mean-Velocity Formulæ,
iii. Discovery of a good approximation to Mean-Velocity,

} for Large Canals.

These are matters of great practical importance: their investigation has been kept steadily in view throughout. In fact the Work, as a whole, may be said to lead up to this end: a few collateral questions have

* By the Executive Engineer, N. Divn., Ganges Canal.

† The late Lieut.-Col. J. Dyas, R.E., Lieut. J. Carroll, R.E., Lieut. P. Cotterill Smith, R.E.

‡ This information is from the MS. Reports deposited with the N. W. P. Irrigation Dept.

indeed been taken up, but strictly subordinated to the main end. Results of great interest—besides the main objects sought—might of course be expected to accrue in the course of the regular work.

6. Results.—It may be well to give at once a very brief abstract of the chief Results attained. A very full Summary is given in Chap. XXVI., in which the whole of the chief Results are brought together for ready reference.

6a. MAIN RESULTS.—The following are the *primary* Results :—

"Loaded Tube-Rods give a rapid and sufficiently close approximation to Mean Velocity past a Vertical: they are so handy in use that they should supersede all other Instruments for the purpose (in depths not > 15')",..................(1).

"With good arrangements Discharge-measurements obtained (by the system advocated) under similar conditions may be expected not to differ more than 3 per cent.",..(2).

"None of the known Mean-Velocity Formulæ appear to be of really general applicability. Kutter's appears to be the most general: the approximation likely to be given by it falls far short of that given by direct Discharge-Measurement",..(3).

"Central Mean Velocity-Measurement appears to be the best means of *rapid approximation* to Mean (Sectional) Velocity; but the reduction must (at present) be effected by a coefficient to be *found by previous special Experiment* at each Site",..(4).

The research ending in Result (1) occupies Chap. XV.; the convenience of the Instrument is so great that this Result is considered to be of great practical use. The Method of Discharge-measurement is explained in Chap. XIX.: the Conclusion No. (2) is adopted after a very extended critical Test of the mutual agreement of the Results, given in Chap. XXI. It is submitted that this is—considering the nature of the Work—a highly favorable Result. The Testing of known Mean Velocity-Formulæ occupies part of Chap. XX.; the Conclusion No. (3) must be confessed to be disappointing, especially as the author is not prepared to suggest any improved formula.

6b. MINOR RESULTS.—The following are a few of the more interesting secondary Results :—

"The motion of water in Large Open Channels is *essentially unsteady*: even in long straight fairly uniform Reaches, the velocity at any given point is *extremely variable from instant to instant;* and the stream-lines *interlace freely* in all directions",..(5).

The water-surface is *not sensibly convex* across the channel",..................(6).

"The Average Velocity-Surface is, as a rule, convex throughout",..............(7).

"The Maximum Velocity past any Vertical is, as a rule, *below the surface:* the depression increases from the centre towards the banks (in a rectangular channel). The depression is *not much affected* (if at all) *by wind*",........(8).

"Loaded Rods nearly grazing the bed *move slower* than the Mean Velocity past them: so that to measure Mean Velocity past a vertical, their immersed length should be *decidedly shorter than the full depth*",......................(9).

"The Mean Velocity at a given Site depends more on the Surface-Gradient, (which is determined by the state of Control above and below the Site,) than on the mere depth of water at the Site",..(10).

"A Discharge-Table for a given Site must (by reason of (10)) necessarily be one of at least double entry, showing the Discharge as depending on both Surface-Gradient and Gauge-Reading",...(11).

The great practical importance of Result (5) will appear in Chap. VI., Results (6) and (9) are specially interesting as being contrary to received opinion. Result (11) is important, inasmuch as the (official) Discharge Tables are of single entry purporting to show the Discharge corresponding to a given Gauge-Reading.

6c. General Review.—The General Result of this Work may perhaps be considered in some ways disappointing, in that there are no brilliant Results, no simple laws of fluid motion discovered, not even a new Formula proposed for Mean Velocity; the complexity of fluid motion becoming indeed the more apparent, the more the conditions of experiment are varied. Nevertheless the author submits that, on the whole, the practical objects proposed have been attained (compare Art. 5, 6a); and that, taking this together with the great scientific interest of many of the other Results, the great outlay on this Work has been usefully incurred.

7. Favorable Sites.—In order that a Site may be really favorable for Experiments on the Flow of Water *under simple conditions* (as indicated in Art. 3), *i.e.*, uncomplicated by local peculiarities of the Site, it seems desirable that—

"The Site should be situate in a straight uniform Reach of great length, *i.e.*, with uniform Banks, uniform Bed, and also uniform Bed-slope for a great distance both above and below the Site",.....................................(12),

and that these distances should be sufficient to reduce the motion to a state depending solely on the following local elements—

"Bed-Slope, Figure of Cross-Section, Roughness of Channel, Depth of water",

uncomplicated by the disturbing action of Obstructions, Inlets, Escapes, Falls, &c.

It seems doubtful whether a Site can be found really fulfilling the above favorable condition in any earthen channel (whether natural or artificial) with high velocity, except where artificially protected by masonry, owing to the great liability to scour, which destroys the uniformity of Bed-Slope. In default of finding so favorable a Site, it seems desirable, at any rate, to avoid complication of the motion by Obstructions below the Site, so that,—

"Experimental Sites should not be situated in marked hollows in the Bed-Slope",..(12*a*).

7a. SHELTER.—High wind renders both water-level determination and velocity-measurement more difficult than in calm weather, so that when Experiment is to be carried on continuously for a length of time, a Site sheltered from wind both by high banks and trees is very desirable. In a hot climate a sheltered Site is also desirable, as the shelter from the sun enables Field-work to be carried on longer than it can be at an exposed Site.

8. Vertical, Transversal.—For shortness' sake these two terms will be employed,—

VERTICAL. Any vertical line in a channel extending from the surface to the bed will be called simply a Vertical. The vertical at mid-channel will be called the Central Vertical: all others will be called Non-Central Verticals.

TRANSVERSAL. Any horizontal line across channel will be called simply a Transversal. The most important are the Surface-, Middepth-, and Bed-Transversals.

9. Velocity-Ordinate, -Curve, and -Surface.—The introduction of some geometrical terms will be convenient here for shortness' sake in treating of the distribution of velocities in a channel.

VELOCITY-ORDINATE. A straight line drawn down-stream from any point in a cross-section of a channel, perpendicular to the plane of the cross-section, and proportional to the "forward velocity"* at that point, will be called the VELOCITY-ORDINATE at that point.

VELOCITY-CURVE. The tips of the velocity-ordinates at all points of any straight (or curved) line in a cross-section will trace out a certain curve which will be called a VELOCITY-CURVE, and the line in question will be called the BASE-LINE (or BASE-CURVE) thereof.

The most important Velocity-Curves to be studied are those drawn on vertical and horizontal Base-Lines: these Base-Lines will be called—

BASE-VERTICALS and BASE-TRANSVERSALS,

and the curves drawn on them will be called—

* *i.e.*, resolved part of the "actual velocity" taken parallel to the current-axis, see Ch. IV., 1.

1°, VERTICAL VELOCITY-CURVES; and

2°, TRANSVERSE (or HORIZONTAL) VELOCITY-CURVES.

Of the latter curves the most important are—

(a), SURFACE-; (b), MIDDEPTH-; and (c), BED-VELOCITY-CURVES,

whose Base-Lines are Transversals—

(a), in the surface; (b), at middepth; (c), and across the bed,

the bed being *supposed to be level across* in the two latter cases.

[The middepth line and bed-contour lines are of course usually curved lines, being straight lines only when the bed is level across. The Middepth and Bed-Velocity-Curves would be plane curves only in this same case. This is, however, the only case in which they have been studied in the present Experiments].

Another very important curve, which may—by an obvious extension of the definition—be included in the Transverse Velocity-Curves is the following:—

MEAN VELOCITY-CURVE. This name will be given to the curve traced by the tips of velocity-ordinates at all points of *any* horizontal Base-Line, each proportional to the "Mean Velocities past the Verticals"* through those points.

Again—

VELOCITY-SURFACE. This name will be given to the Surface traced out by the tips of the velocity-ordinates at all points of a cross-section.

It is clear that the Velocity-Curves are,—by their ordinates,—graphic representations of the distribution of the "velocities"† past their Base-Lines, and that the Velocity-Surface is similarly a graphic representation of the distribution of the "velocities"† throughout the whole cross-section. Great use will be made of these curves in the sequel.

Also the Velocity-Curves are—excepting the Mean Velocity-Curve—obviously plane sections of the Velocity-Surface by planes drawn through their Base-Lines perpendicular to the cross-section.

Also the Areas of the Velocity-Curves are—including the case of the Mean Velocity-Curve in a rectangular channel only—the measures of the (superficial) Discharges past their Base-Lines: and the Volume of the Velocity-Surface is the measure of the (cubic) Discharge through the whole section.

[It will be understood that the terms "vertical", and "transverse" apply to the *position of the Curves*, (not to the direction of the velocities, which are of course parallel to the current-axis:) thus these Curves being in vertical and transverse planes may, with propriety, be termed VERTICAL-CURVES and TRANSVERSE-CURVES].

* *i.e.*, means of the velocities at all points of each Vertical, see Chap. IV, 3.

† *i.e.*, "forward velocities", *see* Note on last page, and Chap. IV, 1.

10. Velocity-Curves, Form.—The investigation of the figure and size of the various Velocity-Curves in different positions and under various conditions in the same Cross-Section, viz.,

Vertical Curves upon different Verticals and at various water-levels for each,
Transverse Curves upon different Transversals and at various water-levels for each,

is an object of the very greatest importance; both to THEORY, as being a help towards forming a rational Theory of fluid motion; and to PRACTICE as leading up to one of the most hopeful modes of arriving within a moderate expenditure of time at a fair approximation to (the most important hydraulic quantity, viz.,) the Total Discharge through a given section: it has accordingly always been a matter of great interest.

Much and continuous Experiment was accordingly devoted towards obtaining data for this research.

11. Mode of Research.—The mode of research employed was to measure the "forward velocity"* at many points of the same Vertical, or of the same Transversal with (nearly) constant water-level, thus giving the values of many velocity-ordinates of the same Vertical or Transverse Velocity-Curve: next to effect the same with other (nearly constant) water-levels on the same Vertical or Transversal, thus giving the ordinates of different Velocity-Curves upon the same Vertical or Transversal: next to repeat the process upon different Verticals and Transversals at the same Site; and lastly at different Sites.

12. Field-Work.—The ordinary *systematic* Field-work consisted chiefly of VELOCITY-MEASUREMENT, and this mainly of following kinds:—

i. Velocity-measurements at many points on same Vertical.
ia. Rod-velocity-measurements along with above.
ii. Velocity-measurements at many points of same Transversal, viz.,
(*a*), Surface-Velocities; (*b*), Middepth-Velocities; (*c*), Bed-Velocities;
(*d*), Mean Velocities past many verticals.
iii. Central Surface Velocity-measurements along with ii*d*.

The Results of i, ii when plotted as ordinates to their respective Base-Lines (Verticals or Transversals) gave rise to the various VELOCITY-CURVES above called for shortness,—

i. Vertical Velocity-Curves.
ii. Transverse Velocity-Curves, comprising the following:—
(*a*), Surface-; (*b*), Middepth-; (*c*), Bed-; (*d*), Mean-Velocity-Curves.

* See Chap IV, 1.

The rest of the *systematic* Field-work was mostly subsidiary to the Velocity-work above, and consisted chiefly of—

iv. Surface-Slope Measurement.
v. Levelling, in connexion with the last.
vi. Sounding, to determine the Average Cross-Section of Channel.

A very great deal of each of these six kinds of Field-work was done in the course of the Experiments. The description and discussion of Nos. i—iv, which are the chief hydraulic data, form the bulk of this work.

Besides the above systematic work, occasional Experiments were also made on many other points of collateral interest, and will be found described in due course. The most systematic of these were on the subjects of—

vii, SILT, see Chap. XXIV., and viii, EVAPORATION, see Chap. XXV.

12a. SCALE OF FIELD-WORK.—The extensive scale of the Field-work can now be judged of from the following brief Abstract* of the *systematic* Work only :—

i,.......... 565 Sets of Vertical Velocity-Curve Work, (each containing velocity-measurements repeated 3 times at every foot of depth.)
ia,........543 Rod-Velocity Measurements done along with above, (each repeated 6 times.)
ii*a*, *b*, *c*, 133 Sets of Transverse Velocity-Curve Work, (each containing velocity-measurements repeated 3 times upon from 10 to 21 verticals.)
ii*d*,........581 Sets of Mean Velocity-Curve Work, (each containing velocity-measurements repeated 3 times upon from 10 to 21 verticals.)
iii,..........313 Central Surface Velocity-Measurements, (each repeated 48 times.)
iv,..........440 Surface-Slope Measurements, (about 150 done on both banks.)
vii,........ 90 Silt-Collections.
viii,......... 40 Evaporation-Measurements.

It will be obvious that the number of velocity-measurements was enormous (about 50,000). The occasional Experiments done are not worth detailing here, though forming an important addition to the systematic work.

12b. PRACTICAL OBJECTS.—The Field-work Nos. i, ii just detailed is an obvious Step towards determining the figure of the several Velocity-Curves above-named.

But there was a *very important practical object* kept in view through-

* These numbers are taken (with slight modification not worth explaining here) from Tables 32, & LXXXIV—LXXXVI.

out, (without which end it would indeed probably not have been attempted,)

i & ia. *Velocities past same vertical.* The real object was the Measurement of the Mean Velocity past the Vertical, and finally the discovery of some easy means of rapid approximation thereto.

ii, d. *Mean Velocities past many verticals.* The real object was the Measurement of Cubic Discharge.

iii & iv. *Central Surface Velocity*, and *Surface-Slope.* The real object was the testing of existing formulæ for Mean Velocity.

Although, therefore, a great deal of the discussions in this Work may appear to be of purely scientific interest, and of little practical value, it should be distinctly noted that the Data discussed were obtained almost entirely with the above important practical objects, and that very little time and money have been spent on obtaining Data unnecessary for these practical ends.

[It may be noted that Step No. i is that advocated in the Mississippi Experiments Work (p. 292) as *likely to be the most practically useful* as an initial step in the present state of knowledge, and that it has resulted in showing the Tube-Rod to be an Instrument extremely well suited from all points of view for the purpose].

13. External Conditions.—By this term it is proposed to denote the ensemble of (varying) Conditions which determine the velocities of the water at different points of a Cross-Section, that is to say external to, or *independent of, the Site* itself, and of the state of the channel above and below it. Such are—

1°. Depth of water at the Site.
2°. Regulation of the supply from above.
3°. Regulation of the Discharge below.
4°. Surface-Gradient.
5°. State of wind.

13a. GREAT RANGE REQUISITE.—It is clear that in order to discover the law of dependence of velocity at any definite point upon any of the External Conditions, it is necessary that Experiment should be made on velocities *throughout a great Range* of that Element, *e.g.*, taking the External Conditions in the order of Art. 13,

1°, from very low to very high water.
2°, through a great range of regulation at head of Reach.
3°, through a great range of regulation at tail of Reach.
4°, from very gentle to high surface-gradient.
5°, from a calm to a high wind.

13b. ACTUAL RANGE.—The Range of the External Conditions actually obtained, and also that of the Results consequent, are shown in

Abstract Table 32 for each of the different sorts of *systematic* velocity-work (Art. 12) undertaken. The Range of the Conditions, and, therefore, also of the Results, was in some cases *very great indeed;* especially in the case of the most important kind of work (Mean Velocity- and Discharge-Measurement) at the two principal Sites—

"Soláni Embankment Main", and "Soláni Right Aqueduct",

see Table 32. This makes the present Experiments *very valuable*, no systematic Experiments with such a wide Range of External Conditions having been hitherto published.

[The two Sites in question were both (see Chap. III, and Plate II.) in channels of a highly artificial character, viz.—

Embankment. Clay bed with brick and boulder bars, with masonry stepped sides.
Right Aqueduct. Rectangular masonry section.

It is unfortunate that circumstances* did not permit of equally extended Experiment in any of the Earthen Channels; the Range of External Conditions, and, therefore, also of Results, actually obtained in them was *comparatively* small, *see* Table 32. An extensive Series of Experiments in Earthen Channels is, therefore, still a desideratum].

14. **Range.**—This short term will be used with following meaning:—

RANGE (δ) = Difference *between highest and lowest values* of a number of quantities (of same kind), ...(13).

The Range of the quantities in every Sub-Column throughout the Detailed Tables is entered in the last line but one (marked δ) of each Series or Table. In the Abstract Tables, the "Range line" is the second of the two lines appropriated to each Series.

15. **Measurement.**—The terms Velocity-measurement, Discharge-measurement, Slope-measurement, &c., will frequently be used to denote the more or less imperfect measures of the several quantities Velocity, Discharge, Slope, &c., obtained by experiment, as distinguished from the real values thereof. For shortness' sake, however, the simple words Velocity, Discharge, Slope, &c., will frequently also be used, when the distinction (if required) can be readily supplied by the reader.

16. **Notation.**—The following symbols are used, as a general rule, with the meanings below, throughout the whole Work. They are occasionally also used with other special meanings which are explained as required. Many other symbols are also occasionally used; their meanings are explained as required.

* Chiefly the question of expense.

SYMBOL.	MEANING.
z	Any depth below surface.
Z	Depth of maximum velocity past any vertical.
h	Gauge-Reading, Depth above datum, &c.
H	Central Depth, Depth on any vertical, &c.
R	Hydraulic Mean Depth.
k	Average depth of Obstruction at Falls at Tail of Reach.
l	Length of Connector, Length of Rod.
n	Number.
ζ	Value of Z ÷ H.
b, B	Surface-breadth, Wet Border.
A	Area.
p	Parameter of velocity-parabola.
F_1, F_2, F_3, F	Surface Fall in Upper, Middle, Lower Sub-Reach, and whole Reach.
S	Local Surface-Slope.
v, v_z, v_y	Velocity in general, or at depth z, or at abscissa y.
$v_\sigma, v_{\frac{1}{2}H}, v_H$	Surface, Mid-depth, Bed Velocity (on any vertical).
v_o	Central Velocity, also Central Surface Velocity.
U	Mean Velocity past a vertical, or past a transversal.
$U_\sigma, U_{\frac{1}{2}H}, U_H$	Mean Surface, Mean Mid-depth, Mean Bed Velocity.
U_o	Central Mean Velocity.
V	Maximum Velocity (on any vertical).
$\mathbf{V}$	Mean (Sectional) Velocity.
u	Rod-Velocity.
u_m	Value of $\frac{1}{4}(v_o + 3v_{\frac{2}{3}H})$.
w	Value of $100 \cdot \sqrt{RS}$.
D	Discharge past a vertical or past a transversal *in sq. ft. per sec.*
$\mathbf{D}$	Cubic Discharge *in c. ft. per sec.*
Q	Withdrawal by Distributaries *in c. ft. per sec.*
C, C_b, C_k	Value of $\mathbf{V} \div w$ by Experiment, by Bazin's Rule, by Kutter's Rule.
$\mathbf{c}$	Value of $U \div v_o$, or of $\mathbf{V} \div v_o$.
$\mathbf{c}_b$	Value of 100 C ÷ (100 C + 25·34), (Bazin's Rule for $\mathbf{c}$).
c	Value of $\mathbf{V} \div U_o$.
σ, σ_o	Silt-Density on any vertical, Central Silt-Density *in grs. per c. ft.*
s	Mean Silt-velocity past any vertical *in grs. per sq. ft. per sec.*
$\mathbf{S}$	Total Silt-Discharge *in lbs. per sec.*

17. Units.—The units adopted throughout this Work are—where not otherwise stated—the British foot, and the second. Feet and inches are indicated by the usual single (′) and double (″) accents. Thus,—

Velocities are measured *in feet per second.*
Superficial Discharges, *in square feet per second.*
Cubic Discharges, *in cubic feet per second.*

18. Transliteration.—In transliterating names of places, and Hindústání words, the Hunterian system has been followed, except in words that are *in familiar use* in other spelling. It must be remembered that the orthography of the vernacular names of small places, such as frequently occur in this work, is not always itself well settled, so that different writers spell them slightly differently even when using the same transliteration-system.

19. Text, Tables, Plates.—The Details necessary for the full elucidation of the Experiments are so very heavy, that it was thought best to divide the Work into three Volumes—

VOL. I, TEXT; VOL. II, TABLES; VOL. III, PLATES.

19a. VOL. I., TEXT.—The Text contains the whole of the Description of the Experiments, the Discussion thereon, and the Results. The only Tables which appear in the Text are such as are either, 1° of a descriptive nature, or 2° of a special kind only bearing on a particular point, or 3° very short Tables. All purely Experimental Data are relegated to Vol. II, (Tables.)

The Text is naturally divided into four very distinct Parts,—

Part I., Chap. I-VIII—Preliminary and Miscellaneous.
Part II., Chap. IX-XVI—Vertical Velocity-Curve Work.
Part III., Chap. XVII-XXII—Transverse Velocity-Curve Work.
Part IV., Chap. XXIII-XXVII—Subsidiary Work, and Conclusion.

Each Chapter of the Text is divided into ARTICLES, the subject matter of which is briefly stated at the beginning. RESULTS are printed in a smaller type and *indented* so as to catch the eye. Articles of minor importance, or containing great detail are also printed in small type, (not indented.) Explanatory matter of minor importance is printed in small type enclosed between square brackets [ ], and not indented.

19b. VOL. II., TABLES.—These are divided into two Parts, viz.,

Part I, Detailed Tables; Part II, Abstract Tables.

Detailed Tables.—These contain the whole of the numerical details of the Experiments, *i.e.*, the whole of the data, and also such detailed Results as depend directly on them. These are, therefore, the real primary EVIDENCE in hand.

Abstract Tables.—These contain an Abstract of the principal Data and Results (chiefly "Means" and "Ranges") from the Detailed Tables, together with many additional (computed) Results deduced from them.

Reference to these (Abstract) Tables will *suffice for most purposes* of illustrating the Discussion, and so *save reference to the larger* (Detailed) Tables.

[*Note.*—The Tables are preceded by a general Introduction (of only 4 pages) explaining their Arrangement and Notation. This should be read before consulting them, as it is essential to a clear comprehension of them. The two Sets of Tables have separated numbering and pagination, and are each preceded by a complete Table of Contents].

19c. VOL. III., PLATES.—Very free use has been made of graphic illustration to exhibit whole groups of related Data and Results in a form which can be quickly taken in by the eye. In many cases a reference to the Plates will *save the necessity of a laborious study* of the Tables.

[The Plates are preceded by a general Introduction (of only 3 pages) explaining their Arrangement and Notation. This should be read before consulting them, as it is essential to a clear comprehension of them. The Plates are preceded by a complete Table of Contents].

20. References.—To facilitate reference, the mode of division and numbering of the Text, Tables and Plates, and of the several sub-divisions and parts thereof are given below.

20a. References, TEXT.—The numbering of Chapters, Articles, Clauses, Results, &c., is as follows:—

Chapters, in Roman numerals, as VI.
Articles, in black letter Arabic, as **14**, *at beginning* of Article.
Subordinate Articles in black letter Arabic, with letters, as **14a**, *at beginning* of Article.
Clauses, in two styles, as iv, or 4°, *at beginning* of Clause.
Results, in Arabic figures in brackets, as (25), *at end* of Result.
Subordinate or *Connected Results*, in Arabic figures with letters in brackets, as (25*a*), *at end* of Result.

The numbering of Articles and Results is continuous through any one Chapter, and starts fresh with each Chapter. In references within a Chapter, the Chapter No. is not quoted: in references to another Chapter, the Chapter No. is always quoted, thus—

Art. 14—iv, or 14—4°, denote Article **14**, Clause iv, or 4° within the Chapter.
Result (25*a*), or Eq. (25*a*), denote Result (25*a*), or Equation (25*a*) within the Chapter.
Ch. VI, 14—iv; Ch. VI, 14—4°; Ch. VI, (25*a*), denote the same as above in Chapter VI.

20b. References, TABLES.—The two Parts of the Tables bear separate numbering, thus—

DETAILED TABLES, Nos. I—LXXXVI, ABSTRACT TABLES, Nos. 1—34, so that for purposes of reference,—

Tab. LXX denotes Detailed Table LXX; Tab. 30 denotes Abstract Table 30.

In most of these Tables, the Series, Columns, Sub-Columns and Lines are distinguished as follows :—

Series, numbered in black letter, as 107.
Columns, numbered in black letter, as 6.
Sub-Columns, some marked by special letters, as *q*, *m*, *h*, H, *b*, B, A, R, *v*, *u*, U, D, V, D, &c.,
some by Arabic figures, as 41½, 30, &c.
Lines, some marked by special letters, as *δ*, *v*, *v'*, △, &c.,
some identifiable by the dates given, as 9-1-'79.

The abbreviations Ser., Col., Sub-Col., L., will be used for Series, Column, Sub-Column, Line, respectively in making references.

20c. References, PLATES.—The numbering of Plates and Figures is as follows :—

Plates, numbered consecutively in Roman numerals, thus I—LII.
Figures, some numbered in various styles specially for each Plate,
some bear the Serial No. in black letter quoted from the Tables.

Thus, for purposes of reference,—

Pl. XIX, iii*a*, denotes Figure iii*a* of Plate XIX.
Pl. XX, 9, denotes Figure 9 of Plate XX.
Pl. XXXI, Ser. 109, denotes Figure marked Series 109 on Plate XXXI.

21. Works of reference.—Subjoined are two Lists of recent Works on Hydraulics (chiefly on Flow of Water) which may be found convenient for reference—

I. Reports of the more important Hydraulic Experiments (on Flow of Water), published since 1850.

II. Works on Hydraulics published since 1860, most frequently consulted in preparation of this Work.

List No. I alone aims at tolerable completeness. For convenience of reference, short Titles are given of the Works most frequently quoted in the Text. Many other Works (both experimental and theoretical) have been consulted in preparation of this Work, and are quoted as required.

LIST I.—*Experiments on Flow of Water.*

TITLE, and Season of Experiment. Author; Place and Date of publication.		Short Title.
Traité de la Mesure des Eaux Courantes, ... Boileau, P.;	'45—'52 Paris, '54	Boileau's Expts.
Lowell Hydraulic Experiments, ... Francis, J. B.;	'47—'52 Boston, '55	Lowell Expts.
Recherches Expérimentales relatives au mouvement de l'eau dans les tuyaux, ... Darcy, H.;	'49—'51 Paris, '57	Darcy Expts.
Report upon the Physics and Hydraulics of the Mississippi River, ... Humphreys, A. A., & Abbot, H. L.;	'51—'60 Philadelphia, '61	Miss. Report.
Recherches Expérimentales sur l'écoulement de l'eau dans les canaux découverts, ... Darcy, H., & Bazin, H.;	'55—'60 Paris, '65	Bazin Expts.
Die Internationale Rheinström Messung, ... Grebenau, H.;	'67 Munich, '73	Rhine Comm.
Survey of the Northern and North-western Lakes, ... [In Reports of Chief of Engrs. U. S. A., viz., Report of '68, p. 949, Appx. Wd.; Report of '69, p. 562, Appx. XA; Report of '70-71, p. 554, Appx. AA, D.] Henry D. F.;	'67—'69 Washington, '69—'70	Lake River Expts.
Hydraulics of Great Rivers, ... Révy, J. J.;	'70—'71 London, '74	Révy Expts.
Beiträge zur Hydrographie des Königreich's Bohmen, ... Harlacher, A. R.;	'71—'72 Prague, '72—'74	Elbe Expts.
Theory of Flow of Water in Open Channels, ... Gordon, R.;	'72—'73 Rangoon, '75	Irrawaddi Report of '75.
Improvement of Rivers and Harbours in the States of Connecticut, &c., ... [Report of Chief of Engrs. U. S. A. for '75, Part II, p. 300, Appx. AA 14]. Ellis, T. G.;	'71—'74 Washington, '75	Connecticut Report of '75.
Report of the Surveys and Examinations of the Connecticut River, ... [Report of Chief of Engrs. U. S. A. for '78, Part I, p. 248, Appx. B. 14.] Ellis, T. G.;	'71—'74 Washington, '78	Connecticut Report of '78.
Verslag aan den Koning, ... Heemskerk;	'73—'75 The Hague, '76	[Not quoted].
Hydraulic Experiments at Roorkee, ... [Prof. Papers on Indian Engng. Vol. IV. of '75, No. 18A]. Cunningham, A.;	'74—'75 Roorkee, '75	1874-5 Report.
Report on the Irrawaddi River, —— Part III., Hydraulics of the Irrawaddi, ...	'69—'79	Irrawaddi Report of '79.
—— Part IV., Hydraulic Works connected with the Nawoon River, ... Gordon, R.;	'72—'78 Rangoon, '79—'80	Nawoon Report.

LIST II.—*Other Works on Hydraulics most consulted.*

TITLE, Author, Place and Date of publication.	Short Title.
Report on the Ganges Canal Works, ... Cautley, P. T.; London, '60	Ganges Canal Report.
Etudes théoriques et pratiques sur le mouvement des eaux, ... Dupuit, J.; Paris, '63	Dupuit's Studies.
Motion of Water in Canals, (Transl. of French Acad. of Sci. Report on Bazin's Open Channels), ... [Profl. Papers on Ind. Engng. Vol. V of '68, p. 272]. Anderson, J. C.; Roorkee, '68	Anderson's Motion of water.
On the Steady Flow of a Liquid, ... [Lond., Edin., and Dublin Phil. Maga. Vol. XLII of '71, pp. 184 & 349; Vol. XLIV of '72, p. 30]. Moseley, H.; London, '71, '72	Moseley's Steady Flow.
Fragment containing a Discussion of a New Formula for the Flow of Water in Open Channels, ... Gordon, R.; Milan, '73	Gordon's New Formula.
Gauging of Rivers, ... [Franklin Inst. Journ., Vol. LXV of '73, pp. 305 & 382; Vol. LXVI of '73, p. 24]. Abbot, H. L.; Philadelphia, '73	Abbot's River Gauging.
Discussion des Expériences les plus récentes sur la distribution des vitesses dans un courant, ... [Annales des Ponts et Chaussées, Vol. X of '75, p. 309, Bazin, H.; Paris, '75 and Profl. Papers on Ind. Engng., Vol. VI of '77, p. 55, (transl.)] Cunningham, A.; Roorkee, '77	Bazin's "Discussion".
Hydraulic Manual and Statistics, ... Jackson, L. D'A.; London, '75	Jackson's Manual.
On River Gauging and the Double Float, ... [Van Nostrand's Eclectic Engng. Maga., Vol. XIII, p. 99]. Robinson, S. W.; New York, '75	Robinson's River Gauging.
The New Formula for Mean Velocity of Discharge of Rivers and Channels, ... Kutter, W. R., transl. by Jackson, L. D'A.; London, '76	Kutter's New Formula.
Recent Experiments in the Flow of Water in Rivers and Canals, ... [Van Nostrand's Eclectic Engng. Maga., Vol. XVI, p. 521]. Bornemann, K. R.; Transl. Anon.; New York, '77	Bornemann's Review.
Canal and Culvert Tables, ... Jackson, L. D'A.; London, '78	Jackson's Tables.
Professional Papers on Indian Engineering, ... Various, Roorkee	Prof. Papers on Ind. Engng.
Van Nostrand's Eclectic Engineering Magazine, ... Various, New York	Van Nostrand's Maga.

CHAPTER II.

HISTORY.

Preface.—The reader who is not interested in the History of this Work should read only Art. 1, 3, 4, 5.

1. Origination.—The Design of this Work was conceived by the author when an Assistant Principal in the Thomason C. E. College.*

It appeared to him that there was a great opportunity ready to hand in the presence of a grand Canal—the Ganges Canal—close by, requiring only a moderate Expenditure in Establishment and Plant for conducting the actual Experiment, *if proper superintendence were continuously available.* Accordingly he proposed to Government a small scheme for Experiments, and volunteered to superintend it himself (in addition to his College work:) this scheme was subsequently extended from time to time, according as appeared possible, *i. e.*, within what seemed likely to receive sanction.

2. Field-work, History.—The next few Articles (2a—e) are devoted to a brief History of the Field-work, lasting in all about 4 years. An Abstract of the Cost will be given in Chap. XXVII.

2a. *Preliminary Experiments.*—Only a small grant of money (Rs. 1,750) was asked for in the first instance for Experiments in the cold weather of 1874-75.

A single† Field-party was made up : Field-work was begun on 9-12-'74, and carried on almost daily, till the 15-4-'75, soon after which the Staff was broken up. The first month was employed in learning how to Experiment. The remaining three months were devoted to systematic velocity-work, viz.—

1°. Comparison of Double-Floats, (3 patterns.)

2°. Central Vertical Velocity-Curve Work.

3°. Comparison of Rod-velocity with Mean Velocity past a vertical (given by Double-Floats)—done along with No. 2°.

4°. Surface Velocity Curve Work.

5°. Mean Velocity Curve Work.

The Field-work of this season was confined to the‡ Soláni Sites, and chiefly to the Soláni Twin Aqueducts.

* At Roorkee, N. W. P., India.

† A Field-party consisted of 2 European Observers, and 9 to 13 native petty employés.

‡ For description of Sites, *see* Chap. III.

An Account* of these Experiments was published in July 1875; so far as then known this Report was final.

[The details of most of the Experiments of that Report have been reprinted for completeness' sake in the present Work].

2b. *Season* 1875-76.—The Account of the 1874-5 Field-work was so very favorably noticed† by both British and Foreign Hydraulicians, that the author was encouraged to apply for a further grant of Rs. 3,000 for the cold season of 1875-76.

A single Field-party was again made up: the Field-work was begun in December '75, and carried on almost daily through the cold weather. The work done was of the same nature as before (Nos. 2°, 3°, 4°, 5°), but was extended to embrace—

2°*a*, Non-Central Vertical Velocity-Curve work in Soláni Right Aqueduct;

and was confined as before to the Soláni Sites.

As the season advanced, it became evident that in an ordinary cold weather only a small RANGE of water-level and of the External Conditions could be expected at any one Site, and that consequently Conclusions of only limited value could be expected. As the Work progressed, the advantage to be obtained from carrying it on continuously through a whole hot weather and rainy season so as to obtain Field-work at the extreme stages of high water-level (which is common in the hot weather), and of low water-level (which occurs for a short time in the rainy season) became increasingly evident.

2c. *Season* 1876-77.—The advisability of carrying on the Work *continuously* without breaking up the Staff at the end of the cold season of 1875-76 as before intended was accordingly urged on Government, and a further grant of Rs. 5,500 for the whole year‡ 1876-77 was obtained. The Field-work was accordingly carried on continuously with a single Field-party from December 1875 throughout this year 1876-77.

The Work done was of the same nature as before, but was extended to embrace the following descriptions of Work:—

Nos. 2°, 2°*a*, 3°, 5° at the Soláni Embankment Main Site,

6°, Central Surface Velocity-Measurements along with No. 5°,

7°, Surface-Slope Measurements along with No. 5°,

8°, Silt-Measurement,

9°, Evaporation-Measurement,

but still confined to the Soláni Sites.

2d. *Season* 1877-78.—In this year sanction was obtained for a continuance of the Experiments for *two* years definitely, viz., 1877-78, and 1878-79, with a single Field-party. A sum of Rs. 6,000 (afterwards increased by Rs. 1,650) was allowed for the first year 1877-78. The Work done was chiefly of the kinds Nos. 5°, 6°, 7°; a little only of the kinds Nos. 2°, 2°*a*, 3°, 4° being done to complete work of these kinds previously begun, and all at the Soláni Sites.

* "Hydraulic Experiments at Roorkee, 1874-75".

† *See* "Pioneer" Newspaper of 15-10-'75, pub. at Allahabad;

" "Engineering" of 31-12-'75, and 7-1-'76;

" Van Nostrand's Eclectic Engineering Maga. of May and June '76, pp. 479, 542;

" Comptes Rendus (of French Acad. of Science) of July '76, p. 139;

besides which, letters were received from Mr. H. Bazin, Genl. T. Ellis, and Mr. R. Gordon, (the hydraulicians) encouraging the author to continue the work.

‡ The Indian financial year begins on 1st April; thus the financial year 1876-77 lasts from 1-4-'76 to 31-3-'77.

Early in 1878 the author proposed the advisability of carefully testing the system of Discharge-Measurement adopted, by having *simultaneous* Discharge-Measurements taken at different Sites. Government provided a second Field-party for this special work in Feby. '78 : this work was begun in March, and will be found described in Chap. XXI. A new Site was taken up at the 15th Milestone for this work.

2e. *Season* 1878-79.—The Test-work above-mentioned was carried on into May, after which the second Field-party was broken up. The ordinary Field-work, Nos. 2°—7°, was then resumed at the Soláni Sites.

During the Canal closure (of August and September '78) the Fifteenth Mile Site was entirely remodelled in anticipation of work to be done there in the coming cold weather.

Meanwhile the author proposed the advisability of testing the system of Discharge-Measurement adopted on a yet larger scale by simultaneous Discharge-Measurements above and below the fork of a large Branch Canal (near Jaolí). Government provided two extra Field-parties for this purpose in November '78 ; this Test-work was carried out in January—March '79. This and some other Test-work done by them at the Roorkee Sites is described in Chap. XXI.

A largely increased grant (upwards of Rs. 8,000 in all) was given for this year's work. All the Field-parties were broken up, and the Field-work entirely closed in April 1879.

3. Duration of Field-work.—It will be seen that the Field-work was carried on with a single Field-party at first for 4 months only (Decr. '74 to March '75), and then almost continuously for $3\frac{1}{3}$ years (Decr. '75 to April '79) ; also that during part of this time there were two or three Field-parties at work, viz.—

2 from Feby. to May '78 ; 3 from Novr. '78 to April '79,

so that the FIELD-WORK may be said to have lasted pretty well 4 years. Thus these Experiments are amongst the most extensive ever undertaken.

[In this whole period there were only a few intermissions of Field-work of from a few days to a month, caused partly by stress of weather, partly by sickness of the Observers, partly by the interruption attending the changes of Observers].

4. Observer Staff.—The whole of the more important field-observations of the Experiments, such as—

1°. Accurate determination of water-level.

2°. Accurate laying out of the standard (50′) " Run ", (Chap. IV, 22.)

3°. Use of special Instruments, (*e.g.*, Chronometer, Anemometer, Current-meter, Thermometer, &c).

4°. Use of delicate Surveying Instruments, (*e.g.*, Spirit Level and Theodolite)

was done entirely by thoroughly trained European Observers.

The *regular* " Observers " were all Overseers or ex-Overseers of the Indian P. W. Dept., and had, therefore, already a practical knowledge of surveying. In all the special hydraulic work (such as 1°, 2°), and use of the special Instruments (3°), the Observers were in every case either

trained by the author himself, or by one of the previously trained Observers, (and were in this case examined thoroughly by the author himself before being passed as "Observers":) the author also satisfied himself as to the efficiency of each Observer in the use of the Surveying Instruments before accepting any work from him.

[In a few cases, when from any cause an extra Observer was wanted, the author himself acted as an "Observer". In one special Experiment (Chap. VIII) several of the Staff and Students of the Thomason C. E. College assisted as "Observers"; they were all instructed in their special work by the author himself. With these exceptions the systematic field-observations were done entirely by the Staff of Observers].

The "Observers" were all either Overseers detailed to the work by the Indian Public Works Department, or Non-Commissioned Officers lent from the Bengal Sappers and Miners, or men out of employ, as shown below.

[The distinguishing "Initial" in this and following Tables is the "Initial" by which the several Observers are distinguished throughout the large Tables].

No.	Name.	Initial.	*Date of Joining.	*Date of Leaving.	Service on Expts.	Remarks. [gde. means "grade"]
					Y. M.	
1	Sergt. J. Warburton, R.A.,	W	9-12-74 5-12-75	10-5-75 28-8-77	2-1$\frac{2}{3}$	1st gde. Overseer, & 2nd gde. Supervisor, P.W.D.
2	Corpl. H. Rowe, R.E., ...	R	10-12-75	20-4-75	0-4$\frac{1}{3}$	lent from the Bengal Sappers & Miners.
3	Mr. A. Hall, ...	H	20-11-75	12-12-76	1-0$\frac{2}{3}$	an ex-Overseer of P.W D.
4	Sergt. W. Porters, R.E., ...	P	18-9-76	24-1-81	4-4$\frac{1}{3}$	1st gde. Overseer, P.W.D.
5	Corpl. G. Grey, R.E., ...	G	10-9-77	31-5-78	0-7$\frac{2}{3}$	lent from the Bengal Sappers & Miners.
6	Sergt. J. Tuer, ...	T	19-2-78	1-6-78	0-3$\frac{1}{3}$	1st gde. Overseer, P.W.D.
7	Sergt. G. Reynolds, R.E.,	R	21-2-78	4-3-80	1-0$\frac{1}{3}$	1st " " "
8	Mr. J. Clowsley, ...	Cl	14-11-78	6-5-79	0-5$\frac{2}{3}$	1st " " "
9	Mr. J. Andrews, ...	A	17-11-78	1-5-79	0-5$\frac{1}{2}$	2nd " " "
10	Mr. C. P. Smith, ...	S	16-11-78	1-5-79	0-5$\frac{1}{2}$	2nd " " "
11	Mr. J. Callaghan, ...	C	9-12-78	1-5-79	0-4$\frac{3}{4}$	1st " " "
12	Sergt. J. Bell, ...	B	3-2-79	1-5-79	0-3	1st " " "

[Besides the above, two more men (Mr. J. Chapman, and Mr. G. S. Henry), both ex-Overseers of the P. W. D., were trained as Observers, one in August 1877, and one in March 1878, to fill existing vacancies, but were discharged at their own request before they were fully trained].

The only satisfactory mode of provision proved to be the obtaining Overseers from the P. W. D., as they were the only men whose services could be depended on. The men from the Sappers and Miners were lent

on too precarious a tenure, being liable to sudden removal for regimental duty: this liability led in one case (that of Corpl. Grey) to very serious inconvenience, as will appear hereafter (Chap. XXI). The entertainment of men out of employ proved also very unsatisfactory.

The changes of "Observers" were of course very disadvantageous to the Work, as the new-comers often involved a loss of nearly a fortnight—(occupied in instruction)—of useful Field-work, and the out-goer had always to sacrifice Field-work to completing the "preliminary reductions" (Art. 7) of his own work. The actual *changes* were, however, not so frequent as might appear at first sight, as new-comers joining about same time (*e.g.*, Nos. 6, 7; also Nos. 8, 9, 10) were of course instructed together.

The Observers always worked in pairs, two in each Field-party, according to the following scheme. It will be seen that there was *only one change of the Senior* (the most responsible) of the Observer Staff, viz.,

Sergt. Warburton replaced by Sergt. Porters in August '77;

and as these two had worked together for nearly a year previous to the change, there was a certain continuity of system in petty details running through the whole work.

PERIOD.	Decr. '74 to April '75	Decr. '75 to Oct. '76.	Nov. '76 to Aug. 77.	Sept. '77 to March '78.	March '78 to May '78.		May '78 to Nov. '78.	Decr. '78 to April '79.			March '79.
Number of Field-Party.	No. i.	No. i.	No. i.	No i.	No. i.	No. ii.	No. i.	No. i.	No. ii.	No. iii.	No. iv.
Initial.	W. R.	W. H.	W. P.	P. G.	P. G.	T. R.	P. R.	P. A.	R. S.	Cl. C.	C. B.
NAMES.	Sergt. J. Warburton, Corpl. H. Rowe,	Sergt. J. Warburton, Mr. A. Hall,	Sergt. J. Warburton, Sergt. W. Porters,	Sergt. W. Porters, Corpl. G. Grey,	Sergt. W. Porters, Corpl. G. Grey,	Sergt. J. Tuer, Sergt. G. Reynolds,	Sergt. W. Porters, Sergt. G. Reynolds,	Sergt. W. Porters, Mr. J. Andrews,	Sergt. G. Reynolds, Mr. C. P. Smith,	Mr. J. Clowsley, Mr. J. Callaghan,	Capt. A. Cunningham, Sergt. J. Bell,

5. Superintendence.—The Work, originated by the author as explained, was executed from first to last under his own superintendence. The whole of the experimental details were arranged by him, and closely watchedby him from first to last. Similarly the Reduction and

Publication were carried out under his own eye. Thus the Work has had the advantage of continuity of superintendence throughout.

[From the beginning in 1874 to December 1878 the Field-work lay close to Roorkee, and could be watched by the author in addition to his current duties in College. During the short period when the Work was greatly enlarged by the formation of three Field-parties (Novr. '78 to April '79) arrangements were made to free the author entirely from College work; so that his whole time was then given to their close superintendence. Again, during the first six months' (following the closure of the Field-work) of the Reduction of the Experiments, the author was relieved of great part of his College work, and thereby enabled to devote most of his time to the heavy work of Reduction. With the above exceptions, (*i.e.*, Novr. '78 to Oct. '79), the work of superintendence was done by the author *in addition to his College work*].

6. **Design.**—The Work will be seen to have begun from small beginnings, and to have gradually expanded in scope and importance as time went on. Thus it was not a Work originally planned out as a definite large scheme. This was a decided disadvantage. But it was a disadvantage inseparable from the mode of origination, design, and execution of the work by a comparatively junior officer single-handed, feeling it necessary to ask for funds from year to year according to the encouragement received.

7. **Reduction.**—A certain amount of primary Reduction of the Experimental data, viz.,

1°. Computation of Wet Borders, Areas, Hydraulic Mean Depths, &c.,
2°. Computation of Surface-Falls, Surface-Slopes, Products 100 $\sqrt{RS}$ &c.,
3°. Computation of Velocities, Discharges, Mean Velocities, &c.,
4°. Tabulation of related data of daily work, &c.,
5°. Miscellaneous Reductions,

was regularly done by the Observer Staff; each Field-party carrying out finally the primary Reductions above of its own Field-work. After the closure of the Field-work in April '79, the collation of this vast mass of primary Results, and Reduction thereof to some useful form, had to be done. The two oldest hands of the Observer Staff (Sergts. W. Porters and G. Reynolds) were retained as a "Computing Staff" for this purpose. As the Reduction proceeded, the Results were gradually prepared in the form of Tables and Plates for the Press. At the same time the Text was gradually written up.

[Nearly all the Tables were prepared by the Computing Staff;—the whole of the Plates were drawn by the author himself].

8. **Publication.**—After 6 months' work of Reduction as above, the Tables and Plates were so far advanced (about half finished in MS.)

that it seemed advisable to begin their publication at once without waiting for the completion of the whole Set; the first batch was accordingly handed over to the Press* in Nov. '79. From that time the passing of the Tables and Plates through the Press went on *pari passu* with the Reduction and Preparation for Press, as before. In February 1880 this part of the work was so far advanced that it was found possible to dispense with one of the Computers (Sergt. Reynolds). The other (Sergt. Porters) was retained till the close thereof in December 1880.

Thus the Reduction and Preparation for Press covered 20 months, (May '79 to December '80), *including*, however, the Publication† of the very heavy Tables and Plates.

[The practical advantage gained by this mode of gradual Publication proceeding along with the further Preparation of Results was very great in saving both of time and expense. As a matter of fact, the two Works of Reduction and Publication hardly interfered with each other at all, each actually proceeding nearly as fast as it could have, had the other not been undertaken along with it, and neither singly being sufficient to occupy the whole time of the Computing Staff: so that had the Publication been delayed till the whole of the MS. was ready, a delay of nearly a year (involving of course the salaries of the Computing Staff as Checkers) would have ensued. Many other minor advantages accrued. The only disadvantage entailed is that the actual sequence of the Tables and Plates (published as they were got ready) is not always the best; but this is after all a trifling matter].

9. **Financial Difficulty.**—The times in which this Work was done were peculiarly unfavorable to the ready provision of funds for it. The great famines in Bengal in 1873-74, in Bombay in 1877, and in Madras in 1877 & '78, followed by the Afghan War 1878-80, caused a continued pressure on the resources of the country, which made the provision of Funds for works not of pressing necessity almost impossible after the middle of 1877. From that time forward, the author felt the asking for funds to be a difficult matter.

10. **Services of Observers.**—The work both in Field and in Office was very monotonous, requiring chiefly great patience and attention, and sometimes considerable exposure under the Indian sun.

It is due to the Observer Staff to record that they one and all accepted the work in the proper spirit, and carried out their share cheerfully. The chief credit of the success of the actual Experiment is undoubtedly due to the energy and ability of the two Senior Observers—

Sergt. Warburton, Decr. '74—Aug. '77 ; Sergt. Porters, Sepr. '77—April '79. Many useful hints as to details of the experimental work are due to them.

* The Thomason C. E. College Press.

† The publication of the Text began in Decr. '80.

After the closure of the Field-work, Sergt. Porters remained on as Chief Computer until the close of the Reduction and Publication of Tables and Plates in Decr. '80. To his intelligent revision of the whole Work (Tables and Plates as they passed through the Press, and Text in MS.) many useful details are due, and also much of the general appearance and arrangement of both Tables and Plates.

11. Acknowledgments.—The thanks of the author are due to many officers for much help received in the course of this Work, and especially to—

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CHAPTER III.

SITES.

Preface.—This Chapter contains a detailed description of the Experimental Sites. The reader who is not interested in such detail should pass over Art. 8, 10a, 10b, 11, 12b, 12c, & 14—17; and refer back as occasion requires hereafter.

1. Ganges Canal.—The Ganges Canal—so called because it derves its water from the Ganges—is the largest Canal in Northern India. It is about 350 miles long from its head near Hardwár, (where the Ganges breaks from the great hills of Northern India into the plains,) to its outfall at Cawnpore. In its upper portion it is about 190′ wide and 10′ deep, and discharges about 7000 cubic feet per second at full supply: it decreases gradually in breadth and depth, and volume of discharge—as it parts with water for irrigation—towards its outfall. It gives off at every few miles small Irrigation Channels—technically called Distributaries—of from 20′ wide and under, and in a few places gives off large Branches of 70′ wide and under, which are themselves fair sized Canals.

1a. Canal Reaches.—The slope of the country along the line of the Canal is generally too great for the Bed-slope of a Canal in soil abounding with sand. The Canal is accordingly broken up into Reaches on a gentler bed-slope than that of the country, by a series of Masonry Falls. In what follows the portion of Canal between any two adjacent considerable sources of disturbance, such as Regulating Works or Falls, will be called for shortness a Reach. It is clear that each such Reach is a portion of the Canal quite distinct from the rest, and which may be considered as to all its circumstances of supply at the head, bed-slope, withdrawal at the tail, &c., quite apart from the rest.

1b. *Favorable for Experiment.*—From the great size and large volume of Discharge of the Main Canal near the head, and from the great variety in size, depth, and volume of discharge of its Branches and Distributaries, and from its abounding in long, tolerably uniform, straight Reaches, it affords an eminently suitable opportunity for Hydraulic Experiments both on the large and small scale.

2. Experimental Reaches and Sites.—A brief description of the Sites at which the Experiments were performed, and of the Reaches in which they lie, is given in the Table on next page.

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Canal.	Reach. Reach.	Reach. Head-Works to Tail-Works.	Reach. Length. M. Ft.	Reach. Total Fall of Bed.	Reach. Permanent Obstruction at Tail.	Reach. Reference. Art.	Reach. Reference. Plate	Experimental Sites. Site.	Bed-width.	Max. Depth.	Max. Discharge.	Channel. Bed.	Channel. Banks.	Season of Experiment.	Reference. Art.	Reference. Plate.
Main Ganges Canal.	Roorkee,	Dhanaurí Regulator to Asafnagar (13') Falls,	9, 3562	11'·5	5'	7	I	15th Mile, ..	160'	12'	7000	Earth	Earth	March to May '78, Novr. & Decr. '78, April '79,	8	II, 1
								Solání Embankment { Main Site,	150'	12'½	7000	Clay & boulders	Masonry steps	Aug. '76 to Decr. '78, April '79,	10	II, 2
								Solání Embankment { Minor Sites,	150'	11'¾	7000	Clay & boulders	Masonry steps	Decr. '74 to Jany. '75,	11	XXVIII
								Solání Twin Aqueducts { Left,	85'	10'	3500	Masonry	Masonry vertical	Decr. '74 to April '75, Feby. '77 to Decr. '78, April '79,	12 &c.	II, 3 II, 4
								Solání Twin Aqueducts { Right	85'	10'	3500	Masonry	Masonry vertical	Feb. '75, Decr. '75 to Decr. '78, April '79,	12 &c.	II, 3 II, 4
	Belra, ..	Nírgájní (14') Falls to Jaolí (13'½) Falls,	5, 4099	7'·2	5'½	13	III IV	Belra, ..	180'	11'½	6500	Earth	Masonry slope	Jany. to March '79,	14	IV, 3
	Jaoli, ..	Jaolí Regulator (5') Falls to Chitaura (12') Falls,	5, 2425	6'·3	4'	13	III V	Jaolí, ..	185'	10'½	6500	Earth	Masonry slope	Jany. to March '79,	15	V, 3
Branch Canal.	Kamhera,	Branch Head-Works to Churiyála (9'½) Falls,	10, 2508	18'·3	2'¼	13	III VI	Kamhera, ..	55'	6'	980	Earth	Earth	Jany. to March '79,	16	VI, 3
Distributaries.	Right Jaolí,	Head to Falls,	9, 2530	12'·4	Waterway contracted at every bridge.	17	III IV	Right Jaolí, ..	16'	4'½	190	Earth	Earth	March '79,	17	XLI
	Mansúrpur,	Head to Tail,	12, 1980	15'·4		"		Mansúrpur, ..	10'	4'	80	"	"	" "	"	"
	Míránpur,	Head to Syphon,	5, 3209	3'·2		"		Míránpur, ..	11'	3'½	80	"	"	" "	"	"
	Pimora, ..	Head to Falls,	2, 4520	3'·1		"		Pimora, ..	9'	5'	85	"	"	" "	"	"

Of these Sites, the principal, *i. e.*, those at which the bulk of the *systematic* Experiment was done, and at which also by far the greatest Range of External Conditions (and, therefore, also of Results) was obtained, were the two following Soláni Sites :—

Soláni Embankment Main Site, and Soláni Right Aqueduct Site.

The Soláni Left Aqueduct was used for *systematic* work for a few months only (in 1874-5). An important Series of three months' Experiments were done at the three Lower Sites, (all in earthen channels,) viz.,—

Belra, Jaolí and Kamhera Sites,—

simultaneously; important chiefly as supplying a Test of the Method of Discharge-Measurement. Experiments were done only occasionally at the other Sites, and in general only for some special purpose. A fuller description will, therefore, be given of the six Sites above-named than of the rest.

3. Reaches, PLANS AND LONGL. SECTIONS, (Pl. I, & III to VI).— Plans and Longitudinal Sections of the four Experimental Reaches, (Roorkee, Belra, Jaolí, and Kamhera) are given in Plates I, and III to VI, showing—

1°, positions of the Experimental Sites.
2°, positions and levels of the Floorings of all Masonry Works, (such as Regulators, Bridges, Falls, &c.,) and of the raised Crests of all Falls.
3°, positions and levels of the zeros of all the chief Gauges used.
4°, levels of one Datum Line (or Gauge-zero) at each Site.
5°, rough outline of the Bed, (from levels taken once a year at every quarter mile at mid-channel.)
6°, highest* water-level, plotted from water-levels (which are figured on the Sections) taken at most of the Gauges and Experimental Sites.

[The outline of Bed—No. 5° above—does not pretend to be anything more than a rough outline : the Bed is so rough that it has no really defined level].

Scales. The Plans and Longitudinal Sections are all on the same *horizontal* scale of 1 mile to an inch, except the General Plan (Pl. III) of the Belra, Jaolí and Kamhera Reaches, which is on a scale of 1¾ miles to an inch : the *vertical* scale of all the Longitudinal Sections is 10 feet to an inch.

Mileage. In what follows, both in the Text and Plates, milestones in the Main Canal are numbered from the Head-works of the Canal at Maiápur (near Hardwár), and those in the Anupshahr Branch from the Head-works of the Branch near Jaolí.

4. HIGH BED-SLOPE, SCOUR.—The only permanent levels in the Bed of each Reach are the floorings of the permanent masonry works, (Regulators, Bridges, Falls, &c.) The Bed of the Canal was originally laid out at a uniform slope between these. But this Bed-slope—about

* The highest water-level reached in actual practice.

15″ to a mile near Roorkee—gave rise to such a high current (in the Main Canal) as to cause great scour of the earthen bed and banks, (*i.e.*, of all parts not protected by, or defined by, masonry.)

[This is well seen in the Longitudinal Sections of the Main Canal (Pl. I, IV, V): the outline of the Bed will be seen to be deeply hollowed out between each of the permanent masonry floorings, which are, therefore, generally *high points* in the outline. On the other hand heavy silting is shown in the Anupshahr Branch Canal, especially in the upper part (*see* Pl. VI, 2): but this was due to the channel having been widened out in 1877 (the year before) with the effect of a reduction of velocity through it.

The original Bed-Slope, now often called the "Theoretical Bed", is shown by a clear black line in all the Longitudinal Sections].

In order to diminish in part the effects of this scour, the crests of all the Falls were raised several feet at various dates after 1863; so that all the Falls are now OBSTRUCTED FALLS. Although these raised crests have no doubt diminished the velocity in the neighborhood of the Falls, they have *not caused any considerable* silting up just above them, (*see* the Longl. Sections, Pl. I, IV, V, VI.)

[As the Canal water is often heavily charged with Silt (*see* Chap. XXIV), and generally deposits silt when the current is slack, this is a proof that the water is everywhere *in pretty rapid motion right down to the actual bed*, even when that bed is much (as much as 5′) below the crest of an Obstruction (such as the crest of a Fall, or a Bridge-Flooring). This disposes of the idea sometimes advanced that an Obstruction (right across a channel) causes a still water pool in the Reach above it up to the level of its own crest].

5. Variable Supply, and Control.—The requirements of water for irrigation being very variable according to the season, and state of the weather, the SUPPLY admitted into the Canal is very variable. Again, there is very great power of CONTROL at the Tail of each Reach over the mode of passage through the Reach of the Supply admitted from the Reach above, so that a given Supply may pass quickly (with low level), or slowly (with high level), according to the Control at the Tail.

[Observe that, the terms HIGH SUPPLY, LOW SUPPLY refer to the quantity of water passing, without reference to water-level, whilst the terms HIGH WATER, LOW WATER refer to the water-level without reference to the quantity passing. Thus in consequence of the control,

"High Supply involves High Water, but High Water does not necessarily involve High Supply", ...(1).

"Low Water involves Low Supply, but Low Supply does not necessarily involve Low Water", ...(2).]

5a. DISTRIBUTARIES.—These take their exit from the Main Channel a little above the Tail of each Reach. They can be opened or closed

either wholly or partially according as water is required for irrigation. The opening and closing of them increases or decreases the size of the *Débouchure* or (sum of the Exits) from the Reach, and thereby *lowers or raises the water-level* near the Tail of the Reach, and at the same time *increases or decreases* both the *Surface-slope and velocity* for a given Discharge passing through the Reach.

[The quantity withdrawn by the Distributaries of any one Reach is so small, (compared with the full supply of the Main Channel,) that the above effects are *quite trifling when there is Full Supply* in the Main Channel; but they become relatively important at times of Low Supply, (*see* Ch. VII, 15c)].

5b. Control at Tail.—The Distributaries of any one Reach cannot be fully worked except at high water in the main channel, and not at all at low water. Means are, therefore, provided at the Falls at the Tail of each Reach of maintaining high water (when required) irrespective of the Total Supply of water passing through the Reach.

The Falls are divided into a number of Bays (usually 8, 9, or 10) by masonry Piers. Each Bay separately can be either partially or wholly obstructed by lowering wooden Baulks or "Sleepers" (stretching from pier to pier) on to the top of the permanent masonry crest of the Fall in that Bay, with the effect of *temporarily raising the crest* of that Fall. This of course temporarily *raises the water-level* at the Tail of the Reach, and at the same time *decreases the surface-slope* and *velocity* for a given Discharge passing through the Reach.

[This temporary obstruction is of course unnecessary at Full Supply in the main channel. The amount of such Control possible is very great, being sufficient to almost close the Falls (in times of Low Supply), and so force a large portion of the whole Supply into the Distributaries leading out from above them: this has occasionally happened in the Roorkee Reach, when water was wanted for irrigation near Roorkee, and not much required below; *see* Ch. VII, 15c for examples of from one-half to one-fourth of the whole Supply being forced into the Distributaries].

5c. **Control,** Effect on Experiment.—The great variation both of Supply at different seasons, and of Control over the mode of passage through a Reach of a given Supply admitted from above, have enabled these Experiments to be done under a very wide "Range" of Conditions (*see* Tab. 32) in two distinct ways—

1°, under all conditions of supply, from full supply (about 7000 c. ft. per sec.) to very trifling supply (about 100 c. ft. per sec.) *at the same Site.*

2°, with widely different surface-slope and velocity *for the same water-level at the same Site.*

This wide "Range of Conditions" actually obtained, makes these Experiments very valuable, (compare Chap. I, 13b).

6. Sites, Cross-Sections, (Tab. I to IV, & Pl. II—VI.)—The Average Cross-Sections of the Sites—obtained as explained in Chap. V, 13—are shown in Tables I to IV. Two of the Average Cross-Sections for each Site are shown in Plates II, IV, V, VI, usually those obtained at pretty high and pretty low water-level: these show in addition to the outline of the Bed—

1°. The Datum line to which the Water-Levels are always referred, (by a clear line.)

2°. The Datum line from which the Cross-Sections are plotted, (by a clear line.)

3°. The Level of Bridge-Flooring next below the Experimental Site, (by a line in long dashes.)

4°. The positions of the Soundings (Ch. V, 13) and Float-Courses, (Ch. IV, 19,) (by long and short verticals.)

5°. The level of the Ropes (Ch. IV, 14) used for defining the Run, (Ch. IV, 22,) (by the upper dotted line.)

[No. 1° is either the Gauge-zero when a Gauge was available, or (in absence of a Gauge) either the Original Bed-Level or other convenient Level.

No. 2° either coincides with No. 1°, or else is an arbitrary line at a convenient depth below No. 1° (everywhere below the Bed), so that the "Heights of Bed above Datum" of the printed Tables I—IV, may be all *positive quantities*].

6a. *Bed unfavorable.*—It will be seen that the uniformity of Bed-slope laid down in Chap. I, 7, as one of the conditions necessary to a Site very favorable for Experiment on Motion of Water *under simple conditions*, does not exist now on the Ganges Canal. The scouring out of the earthen Bed into a series of hollows, (between the masonry floorings,) and the presence of Obstructed Falls, makes it difficult to select a Site which shall not be attended with the disadvantage of being in a hollow, *i.e.*, with its Bed depressed below the next Bridge-Flooring, or below the raised crest of the Falls at the Tail of the Reach.

The Experimental Sites of the present Experiments were on the whole favorably situate as regards the last Conditions, their Beds being in general—

1°, not depressed below the next Bridge-Flooring.

2°, not depressed below the raised crest of the Falls at Tail of Reach.

[These Conditions may be gathered from the Cross and Longitudinal Sections, thus—

1°. The level of the Bridge-Flooring next below each Site is shown on the Cross-Section by a line *in long dashes*.

2°. The level of the raised crest of the Falls at the Tail of the Reach is carried back by a *dotted* line to meet the Original Bed-Level (clear line) in the Longitudinal Section of the Reach].

This last state (2°) is, however, liable to be disturbed by the practice on this Canal of *temporarily raising the crest* of the Falls (in stages of Low Supply) by timber baulks, so as to temporarily raise the water-level above for irrigation purposes (Art. 5b).

7. **Roorkee Reach,** (Pl. I.)—The Ganges Canal crosses the Rátmú Torrent, (*see* Plan, *Fig.* 1,) *by a level crossing* at Dhanaurí. The Dhanaurí Works consist of an INLET (on the right bank of the Canal) for admitting the Torrent, and a DAM or ESCAPE (on the left bank of the Canal) with 57 gates, which serve both to control the escape of the upper Canal waters down the Torrent bed at all times, and also to permit the escape of the flood waters of the Torrent; and lastly, of a REGULATOR or Bridge of 10 arches across the Canal, just below, (and forming in fact the right bank of the Torrent,) each arch being furnished with Drop-gates for controlling the entry of the upper Canal waters and Torrent waters into the lower Canal.

There is so much power of Control at all times over the water passing at Dhanaurí, that the lowest of these Works, *i.e.*, the Dhanaurí Regulator, may be fairly looked on as the HEAD-WORKS of the Roorkee Reach. From this point the Reach is quite straight for nearly six miles, (*see* Plan,) *i.e.*, to Roorkee Bridge, and after a slight bend (near the 19th milestone) is quite straight for 3½ miles more, *i. e.*, to the Asafnagar Falls. The upper six mile straight length seems eminently suited for Experiments. This length contains three descriptions of channel, each containing one or more Experimental Sites, viz.—

1°. *Earthen*, (from Head to Mahewar Bridge,) roughly trapezoidal, 2¼ miles long, contains 15th Mile Site.

2°. *Soláni Embankment*, (from Mahewar to Roorkee Bridge.) Clay and boulder bed, with banks of masonry steps, 2¾ miles long, contains the Soláni Embankment (Main and Minor) Sites.

3°. *Soláni Aqueduct*, twin rectangular masonry channels 932′ long, containing the Soláni Twin Aqueduct Sites.

A general description of the Roorkee Reach showing the nature and cross-section of the channel at different parts, the positions of the Experimental Sites, and the levels of all important points, is given in the Table on next page.

PLACE.	Distance from Maiápur. M.	Ft.	Reduced Levels (G. T. S.* Datum.) G. means Gauge. U. means Upper. L. means Lower.		H. W. L. Gauge-Reading.	Description of Channel, (Plan & Longl. Section, Pl. I; Cross Section, Pl. II.) Length.	Bed-width.	Bed.	Banks.	Cross-Section. Description.	Fig.
Dhanaurí Regulator,	13,	328	Floor	866·69	...	about 2¼m.	about 160′	Earth	Earth	Rough trapezoid.	1
„ Gauge, ...	13,	359	Zero	866·69	9·4						
Pírán Kalliar Bridge,	14,	1721	Floor	865·39	...						
15th Mile { New Site,	14,	5250	Datum	858·41	...						
15th Mile { Old Site,	15,	0	Datum	858·41	15·4						
Soláni Embankment											
Mahewar Bridge,	15,	5070	Floor	863·08	...	about 2 m.	150′	Clay, [brick & boulder bars]	Masonry steps	Stepped trapezoid above rectangle.	2
Embkt. Main Site,	17,	449	Datum	861·26	10·1						
Embankment, Minor Sites,	17,	2023	...	?	...						
	17,	2358	...	?	...						
Twin Aqueducts, {	18,	404	Floor	860·09	10·1	932′	85′ each	Masonry	Masonry vertical	Rectangle [rounded corners].	4
	„	„	Zero	860·09							
Roorkee Bridge, ...	18,	3808	Floor	859·67	...	about ½ m.	150′	Clay, [brick & boulder bars]	Masonry steps	Stepped trapezoid above rectangle.	2
Ganeshpur Bridge, ...	20,	74	Floor	858·36	...	about 4 m.	about 160′	Earth	Earth	Rough trapezoid.	...
Asafnagar (13′) Falls, [Crest raised 5′ above bed]	22,	3790	G.Zero	860·29	4·5						
	...	...	U.Floor	855·19	...						
	22,	3890	Crest	860·29	...						
	...	...	L. Floor	847·29	...						

The Longitudinal Section (Pl. I, 2) shows the (rough) outline of the Bed in four different years 1875—78, as determined by levels taken once a year along the centre line of the Bed at every quarter-mile. The Highest Water-Level Line (of canal practice) shown is determined by Readings taken at the Head-Gauge, at three Experimental Sites (as shown on Plate), and at the Tail-Gauge of the Reach.

The Sites will now be described in detail in the order (quoted above) in which they are situate, beginning from above.

8. **Fifteenth Mile Sites,** (Earthen Channel, Pl. II, 1).—Very little systematic Experiment (only 17 Discharge-measurements in all) was done at these Sites, so that they are of minor importance.

Old Site. This Site (*i.e.*, its centre Cross-Section) was at the 15th milestone. A few (13) Discharge-measurements were made at it between March and May 1878, for comparison with Discharge-measurements made *at same time* at the principal (Soláni Embankment and Aqueduct) Sites lower down. The Site was not well suited for the purpose, its bed being very rough, no preparation being possible at the time: it was selected there only as being about the best available at the time.

* The Great Trigonometrical Survey (G. T. S.) Datum is Mean Sea Level at Karáchi.

New Site. In the expectation of further *systematic* Experiment being done at this Site in the cold weather of 1878-79, the Site was specially prepared during the Canal-closure of 1878, by levelling and smoothing the bed for a length of 400′, and by dressing both banks to a roughly uniform slope throughout the same length. The alteration effected was so great, that the Site became practically a *new Site* (*see* Pl. II, 1; the New Site is shown by the dotted line).

[The centre Cross-Section of the Site was also transferred 30′ above the 15th milestone to avoid a masonry drain which was found to interfere with the running of the Floats used, in consequence of its projecting somewhat from the bank (after the alterations)].

Only 4 Discharge-measurements were made after all at this Site, (in consequence of more important work engaging the Staff elsewhere.)

Cross-Sections. The Average Cross-Sections obtained as described in Ch. V, 13, and taken as follows :—

Old Site, 28-3-'78, and 31-5-'78 ; *New Site*, 16-12-'78, and 28-4-'79,
are shown in Table I, and one of each is also shown in Plate II, *Fig.* 1, viz.,

Old Site, 28-3-'78 by a clear line ; *New Site*, 16-12-'78 by a dotted line.

Water-Level. There was no (permanent) Gauge at either Site : the Water-Level was determined always by one of the Temporary Modes described in Ch. V, 4, 7.

Bed-Level. It will be seen (Pl. II, 1) that a part, about 50′ in width of the Bed of the Old Site (clear line) was *below* the level of the Floor of the Bridge (at Mahewar) next below the Site (shown by a line of long dashes) : whilst the Bed of the New Site (dotted line) was wholly *above* that level. Thus the New Site was favorably, and the Old Site was somewhat unfavorably, situate.

9. Solani Embankment, (Pl. I, & II, 2).—The Ganges Canal is carried across the valley of the Soláni River here about 2¾ miles wide, (*i.e.*, from Mahewar Bridge to Roorkee Bridge,) in a lofty earthen embankment—the SOLÁNÍ EMBANKMENT—which is about 2¾ miles long, 242′ wide at top, and 317′ wide at base: the cross-section of the Banks is pretty uniform (Pl. II, 2) for a length of 1¾ miles, (from a little below Mahewar Bridge to the Soláni Aqueduct-Approach,) and again for a length of ½ mile, (from the Soláni Aqueduct-Exit to Roorkee Bridge.)

The Banks throughout these lengths consist of 12 masonry Steps plastered with fine plaster; the lowest (or 12th) step* of 4′ Rise, (above the original bed-level,) and the remaining 11 steps of about 9″ Rise each, and all of 14″ Tread, (*see* Pl. II, 2.)

[The plaster is in many places now worn away ; and the Steps, which were originally on a uniform slope, have sunk a little in many places, and are even broken away in a few places, so that the original strict uniformity of the Banks no longer exists : but they may still be said to be *pretty uniform* throughout].

The Bed between the (lowest) 4′ Steps is 150′ wide throughout. It was made of clay, and was (originally) laid out on a uniform slope : but

* This 4′ Rise of the lowest Step is elsewhere—for shortness—termed the 4′ "drop-wall."

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having been much eroded by the current, it has been protected from further scour by brick and boulder bars at frequent intervals. The Result is that the Bed is now very irregular throughout, (*see* Pl. I, 2, & II, 2.)

9a. Solani Embankment Sites, (Pl. II, 2; & XXVIII).—Experiments were made at three different Sites (viz., one Main and two Minor Sites) within the above-mentioned unbroken length of 1⅞ miles, of which a detailed description is given below.

Water-Level. There is no Gauge at any of the Sites: the Free Water-Level was in all cases found by the Temporary Arrangement described in Ch. V, 7. The general uniformity of the steps admitted of this being done *with great accuracy*, as the water slips by very quietly.

Bed-Level. It will be seen that the Bed (*see* Pl. II, 2; & XXVIII) is a good deal scoured out below the original bed-level (shown by a clear line), and also a good deal *hollowed out on both sides of the centre.*

[This is possibly in part due to the obstruction of the Central Pier of the Soláni Aqueduct (about ¾ mile below the Minor, and nearly 1 mile below the Main, Sites, *see* Pl. I, 1) causing the current to be somewhat slower about mid-channel (*see* any of the Velocity-Curves, Pl. XXVIII; & XXXIV to XXXVI) than on either side of mid-channel—an unusual condition—and thus admitting of greater scour on either side of mid-channel than at the centre].

10. SOLANI EMBANKMENT MAIN SITE (Pl. II, 2).—One of the *principal* Experimental Sites (*i.e.*, one at which much *systematic* Experiment was done) was situate *about the middle* of the above-mentioned 1⅞-mile unbroken length of the Soláni Embankment, (about 1¼ miles below Mahewar Bridge, and 1 mile above the Soláni Aqueduct Central Pier, which are at the head and foot of this unbroken length,) *see* Plan (Pl. I, 1). This Site will sometimes be called for shortness simply the EMBANKMENT SITE.

The Site was selected with some care in 1876, as having as regular a bed—as far as could be ascertained by extensive soundings—as could be found in the neighborhood.

Bed-Level. A reference to either the Longitudinal or Cross-Sections (Pl. I, 2, & II, 2), will show that the Bed is *only very slightly below* the level of the masonry flooring next below, (the Soláni Aqueduct Floor.) This shows that the SITE was well chosen, *i.e.*, not situated in a sensible hollow, (a somewhat difficult condition to fulfil (Art. 6a).

Cross-Sections. The Sections shown throughout the Plates (II, 2 & XXXIV-XXXVI) for this Site are those given by the Soundings of 15-8-'76 (clear line), and 13-11-'78 (dotted line), *see* Art. 10a, b.

10a. *Two Years* (1876—8) *Constant Bed.*—Most of the systematic Experiment at this Site was done within the two year period from August '76 to August '78; the Canal was kept constantly running throughout this period, so that no considerable silting up* of the Site could occur during that time; also the Bed, being here protected (as above-mentioned) by brick and boulder bars, is not liable to much scour. Thus the Bed at this Site would be pretty constant throughout that time. It was, there-

* There were two short intervals of slack water (in October 1877 and July 1878), but it was not thought worth while to repeat the soundings.

fore, considered unnecessary to ascertain the figure of the Bed at frequent intervals, as would have been necessary in an unprotected earthen channel. The Cross-section was accordingly taken only twice in that period, viz.—

1°. On 15-8-'76, under very favorable circumstances, viz., when the water was low, (about 8'½ deep,) and the current consequently slack.

2°. Again on 4-6-'78 under unfavorable circumstances, in deep (11') water, and in a swift current.

The Results of these Soundings are shown in Table I (referred to a common datum), and will be seen to be closely alike, (as close as can be expected in such a rough bed.)

10b. *Bed variable after August* '78.—The Canal was closed for repairs in August 1878; shortly after its re-opening fresh Soundings were taken (on 28-9-'78), and as considerable silting up was found to have taken place—in consequence of course of the slack water—the Soundings were taken at short intervals, (thrice in all,) viz.—

on 28-9-'78, 13-11-'78, and 16-12-'78,

i.e., whenever Field-work was being done at this Site. The Results are shown, referred to the common datum, in Table I. It will be seen that the Silt was gradually scoured away, until about Decr. '78 the Bed was nearly in the same state as before.

11. SOLANI EMBANKMENT MINOR SITES, (Pl. XXVIII).—A few Experiments (on Surface-Velocity Curves only) were done at two Sites within the above-mentioned 1¼ mile unbroken length of the Solání Embankment, about half-way between the 17th and 18th milestones (*see* Plan, Pl. I, 1) in Jany. 1875. The Average Cross-Sections (obtained as described in Ch. V, 13) were found to be so nearly alike, that the Experiments at the two Sites have been combined into a single SERIES as hereafter explained, Ch. VI, 14. The Average Cross-Section of the two Sites is shown in Plate XXVIII.

[From an irregularity in the Surface-Velocity Curve (Pl. XXVIII) for these Sites, it was thought (for explanation *see* Ch. XVII,) that they were too near the Solání Aqueduct Central Pier, only half a mile below. Experiments were accordingly discontinued at them: so few were done that it was not thought worth while to give the details of the Soundings in the general Tables I to IV as for the more important Sites: neither have the Hydraulic Elements for these Sites been computed].

12. Solani Aqueduct, (Pl. II, 3, 4).—The Canal is carried over the Solání River at a height of 25' above the River-bed in a fine masonry Aqueduct-Bridge of 15 arches of 50' span, 1112' long and 192' wide. The Aqueduct is divided into two equal 85' waterways by a central Pier 932' long and 8' wide, except at the two ends (or Pier-heads), which are 30' long and 14' wide.

The enlarged Plan (Pl. II, 3) of the Aqueduct and Approaches shows, with the help of the Table on next page, how the single waterway 150' wide at bed of the Solání Embankment—of section shown in *Fig.* 2—gradually widens to 172' in the 250' Approach to the Aqueduct, which width is continued uniform with vertical sides for a length* of 90', when

* This length was *incorrectly* figured and plotted as 115' in the 1874-5 Report.

it is parted by the 14′ Pier-head into equal 79′ waterways with vertical sides for a length of 30′: these are enlarged by 3′ offsets on each side into 85′ waterways, which are carried of the uniform section shown in *Fig.* 4 for a length of 872′. The Exit below this is precisely similar to the Approach.

[In strictness the whole Embankment, 2¾ miles long across the Soláni valley, is an Aqueduct, and the portion of it crossing the Soláni River is an Aqueduct-Bridge. Local custom, however, has restricted the term Aqueduct to the latter, and Embankment to the whole Work].

Description.	Length.	Waterways. No.	Waterways. Width.	Cross-Section, [see Plate II.]	Sides. Figure.	Sides. Material.	Bed.
Soláni Embankment, [Upper part.]	10,113′	1	150′ at bed.	As in *Fig.* 2.	12 steps, 14″ tread.	Brick masonry plastered with fine plaster.	Clay with boulders.
Approach to Aqueduct,	250′	1	150′ at bed increasing to 172′ at bed.	Similar to *Fig.* 2.	12 steps, treads decreasing.		Clay with boulders.
	*90′	1	172′.	Rectangle.	Vertical.		Bricks laid flat.
Soláni Aqueduct,	30′	2	79′ each.	2 rectangles.	Vertical.		
	872′	2	85′ each.	2 rectangles, corners rounded as in *Fig.* 4.	Vertical, corners rounded.		
	30′	2	79′ each.	2 rectangles.	Vertical.		
Exit from Aqueduct,	*90′	1	172′	Rectangle.	Vertical.		
	250′	1	172′ at bed decreasing to 150′ at bed.	Similar to *Fig.* 2.	12 steps, treads increasing.		Clay with boulders.
Soláni Embankment, [Lower part.]	2,123′	1	150′ at bed.	As in *Fig.* 2.	12 steps, 14″ tread.		Clay with boulders.

The Cross-Section of either Aqueduct is seen (*Fig.* 4) to be a rectangle 85′ wide with the two lower corners slightly rounded off, so that the bed-width is only about 83′·25.

[As originally constructed the two lower corners were rounded off by 3′ quadrants, thus reducing the bed-width to 82′. But a layer of about 9″ of concrete and brick laid flat was afterwards put on the original floor, thus raising the floor-level about 9″, and greatly decreasing the effect of the rounding off of the corners. The two floor levels† are shown in the Cross-Section, *Fig.* 4, by a dotted line (old Floor), and clear line (present Floor)].

* This length was *incorrectly* figured and plotted as 115′ in the 1874-5 Report.

† The Cross-Section given in the former 1874-5 Report is *incorrect*, having been drawn from the old Standard Plates of the Ganges Canal; it shows the original (instead of the actual raised) Floor-level in consequence.

The public roadways overhang the water 3′ for the whole length of 872′, and are corbelled out from a height of 7′·3 above the bed, so that the water-surface is of the full width (85′) only when less than 7′·3 deep: the corbelling attains the full width (3′) at a height of 9′·85 above the bed, so that as the water-level rises from 7′·3 to 9′·85 above the bed the water-surface decreases to 82′; for all depths over 9′·85 the water surface is 82′, (*see* Pl. I, 3).

[This corbelling out prevents any velocity-measurements being effected nearer to the outer walls than 3′: this will be seen hereafter].

The present Bed or Floor is of bricks laid flat, and generally very regular throughout its length: it was sometimes covered with a thin layer of fine silt, and was sometimes quite bare, (as could be seen in clear states of the water.)

[Occasionally there were irregular patches of stones, bricks, lumps of clay, &c., found in parts of the bed, (even at the Experimental Sites,) dropped apparently from laden carts passing on the roadways, and from laden barges: these interfered a good deal at times with the use of Subsurface Instruments].

The outer sides are plastered throughout with fine plaster: the sides of the central Pier were plastered to a height of 6′ only when the Experiments began (in 1874).

[The plastering of the left side of the central Pier (forming the right bank of the Left Aqueduct) was completed to the full height during the Canal-closure of August 1876: the other side (forming the Left bank of the Right Aqueduct) remained untouched whilst the Experiments lasted].

12a. Solani Aqueduct Sites, (Pl. II, 3, 4.)—The two centre sections of the Twin Aqueducts were chosen as the Experimental Sites. It will be seen that they are each at the middle of a channel 872′ long, of very uniform (nearly rectangular) section, the Bed of which is very even.

[This length 872′ is only about 10¼ times the width of each channel: it seems that this length is not great enough to warrant their being described as "uniform channels of great length". There is a want of symmetry of the Horizontal Velocity-Curves for these Sites about the current-axis of each channel—see any of the Plates XXVI, XXVII, and XXIX to XXXIII—which shows that the length of 872′ is not sufficient to establish the symmetry of motion about the axis of each channel which might be expected in a channel *of great length*].

Bed-Level. The present Floor-level (clear line in *Fig.* 4) will be seen to be *above the level* of the flooring of the next Bridge (Roorkee Bridge) below, so that the Sites are favorably situate.

Gauges. There are STANDING GAUGES (described briefly in Ch. V, 9) at both Sites, one on either side of the Central Pier: both Gauges ruffle the water slightly, so cannot be read accurately to the hundredth of a foot. The Left Aqueduct Gauge,

being the one most used for Canal purposes, was usually read whenever Experiments were going on at the other Sites in the Roorkee Reach.

[Soundings were taken occasionally only at these Sites. The depths of water all over the Experimental Sites were found to be generally very nearly the same all over the bed as shown by the gauges, with the occasional exceptions noticed].

12b. LEFT. AQUEDUCT SITE.—This was *the principal* Experimental Site in 1874-5, (*see* 1874-5 Report). Most of the public traffic, both by land and water, used this Aqueduct (in preference to the Right Aqueduct): it was found to interfere so with the Experimental work, that this Site was given up as a principal Site on the resumption of the Experiments in December 1875: from this time Experiments were done at this Site *only when required in conjunction with similar work in the Right Aqueduct.*

[This Aqueduct was closed for repair whilst the Canal was low (but still running) during part of August and September 1876, October 1877, and September 1878, by a dam of clay *at both ends*: (the whole of the water passing through the Right Aqueduct, whilst the closure lasted). The enclosing dams were *not thoroughly removed* on the two former occasions, so that from August 1876 till the Canal-closure of August 1878, this Aqueduct was *partially obstructed*; so that throughout this period less water passed down it at all times than through the Right Aqueduct. This will be seen in Chap. XXI].

12c. RIGHT AQUEDUCT SITE.—This Site was one of the *principal* Experimental Sites throughout the whole period of the Experiments. Most of the systematic Experiment was done at it. No alterations were made in its Bed or Banks throughout the whole period.

Experiments with Left Aqueduct closed. An opportunity occurred three times (in August and September '76, in October '77, and in September '78) for Experiment at this Site under unusual conditions, the whole of the water in the Canal being passed through this channel, in consequence of the Left Aqueduct being closed for repairs. The central surface velocity exceeded 7' per second on one of these occasions, although the water was low (only 4'·73, *see* Tab. XV, Ser. 18).

13. Belra, Jaoli, Kamhera Reaches, (Pl. III.)—Near the village of Jaolí the Canal throws off a large Branch Canal, the Anupshahr Branch, of 55' bed-width and 980 cubic feet per second volume of Discharge at Full Supply. The neighborhood of the Head of this Branch was selected as a suitable place for a Series of Test Experiments hereafter explained (Chap. XXI.) The application of this Test involved simultaneous Experiment at three Sites—

One Site—at Belra—in the Main Canal above the Branch-Head.
One Site—near Jaolí—in the Main Canal below the Branch-Head.
One Site—near Kamhera—in the Branch Canal.

The Reaches in which these Sites are situate will be called the Belra, Jaolí, and Kamhera Reaches. These Reaches will be called collectively the three LOWER REACHES, and the three Experimental Sites therein will be called collectively the three LOWER SITES. Their relative positions, and

the positions of the Experimental Sites in them, are shown in the general Sketch Plan (Pl. III.)

A brief description of the three Reaches showing the positions of the Experimental Sites, and the levels of all important points, is given in Table on next page. The detailed Plans and Longitudinal Sections of the three Reaches are given in Plates IV, V, VI.

13a. *Belra, Jaolí, and Kamhera Sites*, (Pl. IV, V, VI).—One Experimental Site as above was chosen in each Reach: the Sites were to some extent specially prepared (as detailed below under head of each Site) for these Experiments during the Canal-closure of August 1878; but the closure lasted so short a time, that it was found impossible to dress the Bed at any of the Sites. The Beds of all three Sites were accordingly very rough at the time of the Experiments—(as may be seen by the Soundings, Tab. II, III, IV).

Soundings. The three Sites being all in earthen channels, it was considered necessary to find their Average Cross-Sections (Ch. V, 13b) at frequent short intervals: the Soundings were accordingly taken *once a week*. The Results are shown in the Tab. II, III, IV, all referred to a common datum for each Site.

Water-Levels. A Still Water-Gauge had been provided at the centre section of each of the three Sites, (*see Fig.* 3 of each Plate,) by which the Still Water-Level could be read *very accurately*. The Gauge-Zero at the Belra and Jaolí Sites was nearly in the Original Bed-Slope (*see* the Longl. Sections, *Fig.* 2 of each Plate).

14. Belra Site, (Pl. IV).—This was a Canal Discharge Site, *i.e.*, a Site prepared for Discharge-measurement, and for many years used for this purpose by the Canal Staff. The Site was prepared simply by revetting the banks of the earthen channel with "kachchá pakká" brick walls (bricks set in mud) with a batter of 1 in 2, *i.e.*, of 1 horizontal to 2 vertical—for a length of 250′. The Bed was an earthen one, and very rough.

Situation and Bed-Level. The Site is not favorably situate for Experiment, its centre section being (*see* Tab. on p. 40, & Pl. IV) only 743′ below Belra Bridge, the Piers and Towing Paths of which may perhaps affect the motion of the water as far down as the Site, and must certainly modify the state of the Surface-Slope in the neighborhood. Moreover, the Site was in a decided hollow, its bed being so scoured out (*see* Longl. and Cross-Sections, Pl. IV) as to be about 2′ below the level of the Floor of the Bridge (at Belra) just above, and also about 2′ below the raised crest of the Falls at the Tail of the Reach (Jaolí). The Bed was, however, everywhere above the level of the Floor of the Bridge (at Bhopá) next below, (shown by a line in long dashes on the Cross-Section).

[The Longitudinal Section shows that no Site could have been found in this Reach quite free from these disadvantages. This being so, it was thought that the actual advantages of the Site, viz.,

1°, having uniform permanent straight Banks for 250′,

2°, having a still water Gauge,

Canal	Reach: Name	Reach: Length	Reach: Plate	Place	Distance from Head-Works: M.	Distance from Head-Works: Ft.	Reduced Levels (G. T. S. Datum.): Level of	Reduced Levels (G. T. S. Datum.): R. L.	H. W. L. Gauge-Reading	Description of Channel: Length	Bed-width	Bed	Banks	Cross-Section: Description	Cross-Section: Plate
MAIN GANGES CANAL	BELRA	5 m., 4099 ft.	III. IV.	Nírgájní (14') Falls, ...	42	2536	Crest	820·12	...	about 1¼ m.	about 180'	Earth	Earth	Rough trapezoid,	...
					,,	,,	Lower Floor	805 95	...						
				Tail of Nírgájní Lock Channel,	48	1734	Gauge-Zero	808 89	?						
				Belra Bridge, ...	44	784	Floor	804·39	...						
				Belra Site, ...	44	1527	Gauge-Zero	804·23	8·4	250'	180'	Earth	Masonry, Slope 1 in 2	Trapezoid,	IV, 3
				Bhopá Bridge,...	46	2772	Floor	801·02	...	about 4 m.	180'	Earth	Earth	Rough trapezoid,	...
				Jaolí (13'½) Falls, ... (Crest raised 5½' above bed.)	48	1155	Gauge-Zero	804·12	4·8						
					48	1355	Upper Floor	798·74	...						
							Crest	804·24	...						
							Lower Floor	790·72	...						
	JAOLI.	5 m., 2425 ft.	III. V.	Jaolí Regulator (5') Falls,	50	2223	Crest	792·25	...	1948'	185'	Earth	Earth	Rough trapezoid,	...
					,,	,,	Lower Floor	787 88	...						
					50	2344	Gauge-Zero	788·23	7·5						
				Jaolí Site, ...	50	4296	Gauge-Zero	786·99	?	250'	185'	Earth	Masonry, Slope 1 in 2	Trapezoid,	V, 3
				Dukherí Bridge, ...	53	2028	Floor	785·15	...	about 5 m.	180'	Earth	Earth	Rough trapezoid,	...
				Jánsat Bridge,...	54	3620	Floor	783·48	...						
				Chitaura (12') Falls, ... (Crest raised 4' above bed).	55	4448	Gauge-Zero	785 84	3·5						
					55	4648	Upper Floor	781·07	...						
					,,	,,	Crest	785·07	...						
					,,	,,	Lower Floor	778·01	...						
ANUPSHAHR BRANCH.	KAMHERA.	10 m., 2508 ft.	III. VI.	Branch Head-Works,...	0	0	Floor	788·21	...	about 10½m.	55'	Earth	Earth	Rough trapezoid,	VI, 3
					0	18	Gauge-Zero	793·24	2 3						
				Kamhera Site, ...	2	660	Gauge-Zero	786·16	?						
				Kamhera Bridge, ...	3	501	Floor	783·58	...						
					3	? 501	Gauge-Zero	783·44	7·7						
				Salárpur Bridge, ...	6	3147	Floor	780·15	...						
				Sambalhera Bridge, ...	8	3795	Floor	777·09	.						
				Churiyála (9½') Falls,... (Crest raised 2½' above bed).	10	2482	Gauge-Zero	777·40	3·8						
					10	2508	Upper Floor	774·91	...						
					,,	,,	Crest	777·16	...						
					,,	,,	Lower Floor	767·50	...						

3°, being the Site of the Discharge-measurements taken by the Canal Staff, justified using this Site in preference to selecting a new one].

15. JAOLI SITE, (Pl. V).—This Site was newly prepared for these Experiments (during the Canal-closure of August 1878), just like the Belra Discharge Site by simply revetting the Banks of the earthen channel with "kachchá pakká" brick walls (bricks set in mud) with a batter of 1 in 2, *i.e.*, of 1 horizontal to 2 vertical, for a length of 250′. The Bed (of earth) was left untouched, and was very rough.

Situation and Bed-Level. The Cross-Section shows that the Bed had been scoured out to a depth of about 1′·5, but was still above the level of the Floor of the Bridge (at Dukhérí) next below, and also (*see* Longl. Section) above the raised crest of the Falls at the Tail of the Reach (Chitaura). As far as levels go, the Site was, therefore, favorably situate. The Site was otherwise not very favorably situate for Experiment, its centre being (*see* Tab. on p. 40, & Pl. V) only 2078′ below Jaolí Regulator Falls, and being also situate in an unusually wide part of the Reach, (widened out by erosion.) But its actual advantages, viz.,

1°, having uniform permanent straight Banks for 250′,

2°, having a still water Gauge,

3°, being favorably situate as regards levels,

make it on the whole a favorable Site.

16. KAMHERA SITE, (Pl. VI).—This Site was situate in the Anupshahr Branch of the Main Canal about 2¼ miles below its head, (Pl. VI). It was newly prepared for these Experiments (in the Canal-closure of August 1878) by simply dressing the (earthen) banks roughly to a uniform slope for a length of 250′. The bed (of earth) was left untouched, and was accordingly very rough.

Situation and Bed-Level. The Sections show that the Bed of the Site was fairly on the general Bed-slope of the season, and above all masonry Floorings below it, so that it was favorably situate.

17. Distributaries, (Pl. XLI).—A few Experiments (Mean Velocities, and Central Surface-Velocities only, detailed in Tab. LVI, LXX)—meant solely as a check on the official Discharge Tables—were made in the four Distributaries, which leave the Main Ganges Canal in the Belra Reach above Jaolí Falls (Pl. III, IV).

These channels are simply earthen channels of trapezoidal section, and were in good order at the time of the Experiments, the Beds and Banks being pretty well dressed. The Average Cross-Sections are shown in Table LVI and Plate XLI. The positions of the Gauges are detailed in Table LVI.

CHAPTER IV.

VELOCITY-MEASUREMENT.

Preface.—Most of this Chapter is taken up with the practical and other detail conected with the use of Floats (Art. 13—37). The reader who is not interested in such full detail should pass over Art. 13—18, 23—25, 28, 30, 33—35.

1. Velocity.—This term (when taken alone) is commonly used in Hydraulics *in a technical and limited sense* very different to its proper meaning: this should be understood from the outset. The following terms will be used with the meanings given:—

CURRENT-AXIS. The intersection of a mid-channel vertical plane perpendicular to a cross-section of the channel with the water-surface.

ACTUAL VELOCITY. The actual rate of motion, *i.e.*, *the* "velocity" (in the proper sense) in the actual line of motion, (which may be in any direction, and subject to constant change.)

FORWARD VELOCITY.* The *resolved part* of the Actual Velocity *parallel to the Current-axis.*

VELOCITY. This term (by itself) will be used usually in the sense of (and as an abbreviation for) Forward Velocity.

The FORWARD VELOCITY is the only part of the ACTUAL VELOCITY of much use in practical questions, such as calculations of DISCHARGE, &c.: from this it has come that the term VELOCITY (taken by itself) is now ordinarily used in the sense of FORWARD VELOCITY in Works on practical Hydraulics, (though this is seldom distinctly stated;) and it will accordingly be so used in this Work.

[Convenience alone justifies the use of the term in this limited sense; the term is of such constant occurrence, that the use of any periphrasis would be wearisome].

1a. *Particular velocities.*—It is convenient to couple with the term velocity the name of certain of the chief verticals and transversals of a cross-section to denote the "forward velocity" at any point of the named vertical or transversal. Thus the following terms have the meanings given:—

Central Velocity, { Velocity past any point of the centre vertical, or at centre of any transversal.

Edge-Velocity, ... Velocity at edge, or at any point of edge.

* This term is adopted from Prof. Jas. Thomson's Paper "On the Flow of Water, &c.," at p. 115 of Proceedgs. of Royal Socy., No. 191 of 5-12-'78.

Surface-Velocity, *Middepth-* „, *Bed-* „ } Velocities past the top, middepth, or foot of any vertical, or at any point of the surface, middepth- or bed-transversal.

2. Discharge, SUPERFICIAL and CUBIC.—For shortness' sake the following terms will be used:—

SUPERFICIAL DISCHARGE. The quantity of water passing a straight line (say a Vertical or Transversal) drawn in the Cross-Section of a channel, being obviously a superficial quantity (measurable *in square feet per second*), will be styled the SUPERFICIAL DISCHARGE PAST THE VERTICAL or TRANSVERSAL respectively, and will be denoted by D.

[The most important cases of Superficial Discharge past a Transversal are those past the Surface, Middepth, and Bed Transversals: these will be styled shortly—
Surface-Discharge, Middepth-Discharge, and Bed-Discharge].

CUBIC DISCHARGE. The quantity of water passing through the Cross-Section of a Channel being obviously a cubic quantity (measurable in cubic feet per second) will be styled the CUBIC DISCHARGE, and will be denoted by **D**.

[This latter quantity (being pre-eminently the most important quantity in practical Hydraulics) is commonly called simply *the* DISCHARGE].

For shortness' sake the distinctive terms SUPERFICIAL, PAST THE VERTICAL OR TRANSVERSAL, and CUBIC will frequently be dropped when the context sufficiently shows what is meant.

3. Average and Mean Velocity.—It becomes necessary here to distinguish between several different sorts of "means" of a number of velocities commonly confused together under the single name "Mean Velocity", for which short distinctive names are much required. The new names proposed are five in number, the term "velocity" being always used in the technical sense of "Forward Velocity".

i. AVERAGE VELOCITY. The average or mean of many successive "forward velocities" *at one and the same point* in a cross-section.

ii. FLOAT VELOCITY. The mean of the "forward velocities" *at all points of a straight line* of given length drawn *parallel to the current-axis* through any point in a cross-section.

iii. MEAN VELOCITY (PAST A VERTICAL). The mean of the "forward velocities *at all points of any Vertical* in a cross-section.

[The most important case is the Mean Velocity past the Central Vertical: for shortness' sake this will be styled the CENTRAL MEAN VELOCITY].

iv. MEAN VELOCITY (PAST A TRANSVERSAL). The mean of the "forward velocities" *at all points of any Transversal* in a cross-section.

[The most important cases are—
Mean Surface-Velocity, Mean Middepth-Velocity, Mean Bed-Velocity].

v. MEAN (SECTIONAL) VELOCITY, MEAN VELOCITY. The mean of the "forward velocities" *at all points of a cross-section.*

The great difference *in kind* between several of these Means will be obvious, thus—

No. i is an average (with respect to time) of velocities *at one and the same point.*

Nos. ii to v are means of velocities *at different points.*

No. ii is the mean of velocities *at all points in the line of motion* itself.

Nos. iii and iv are means of velocities *at all points of certain lines in a cross-section.*

No. v is the mean of velocities *at all points of a cross-section.*

Using the usual notation, viz.,

x, y, z for co-ordinates; x lengthways, y transverse, z vertical,
H for depth, b for breadth, A for area; v for velocity, t for time,

the above Means are expressed symbolically as—

$$\left.\begin{array}{l} \text{No. i, } \int_0^t v dt \div t, \text{ No. ii, } \int_0^x v dx \div x, \ldots\ldots \\ \text{No. iii, } \int_0^H v dz \div H; \text{ No. iv, } \int_0^b v dy \div b; \text{ No. v, } \int_0^b \int_0^H v dy dz \div A, \end{array}\right\} (1).$$

Nos. iii and iv will be denoted by U with appropriate subscripts explained hereafter. No. v will be denoted by **V**.

The great convenience of short distinctive names for these quantities will appear abundantly throughout this Work.

[The name of No. ii is chosen from its being the particular mean always given by all sorts of Floating Instruments. It is much to be wished that shorter and yet sufficiently distinctive names could be found for No. iii and iv: to avoid the tedium of the periphrases as much as possible, the short term "Mean Velocity" will be used for both whenever the context suffices to prevent confusion with No. v. The last (No. v) being by far the most important quantity in practical Hydraulics, it has been considered imperative to preserve the use of the short term "MEAN VELOCITY" with this meaning (No. v) in which it has become familiar: when necessary for distinction, it will be termed "Mean Sectional Velocity"].

4. Mean Velocity, COMPUTATION.—The three MEAN VELOCITIES proper, viz.,

No. iii, past a vertical; No. iv, past a transversal; No. v, through a section, have throughout this Work invariably been computed from their fundamental formulæ, viz.,

$$\left.\begin{array}{l}\text{Mean Velocity}\\ \text{past a vertical,}\end{array}\right\} U = \frac{\text{Discharge past the vertical}}{\text{Depth on that vertical}} = \frac{D}{H}, \ldots\ldots (2a),$$

$$\left.\begin{array}{l}\text{Mean Velocity}\\ \text{past a transversal,}\end{array}\right\} U = \frac{\text{Discharge past the transversal}}{\text{Breadth of the transversal}} = \frac{D}{b}, \ldots\ldots (2b),$$

$$\text{Mean Sectional Velocity, } \mathbf{V} = \frac{\text{Cubic Discharge}}{\text{Area of Cross-Section}} = \frac{D}{A}, \ldots\ldots (2c),$$

which will be admitted to be the proper mode of obtaining them, provided of course that the Discharge-measurements on which they depend are sufficiently approximate.

4a. Mean Velocity, ERROR.—It will appear in the sequel that the

Superficial and Cubic Discharge-Measurements depend *virtually* on two and three factors respectively, thus—

Discharge-Measurement.	Factors contained.	Formula.
Past a vertical, ...	One depth-factor (H), and one velocity-factor (say U), ...	U . H
Past a transversal,...	One breadth-factor (b), and one velocity-factor (say U), ...	U . b
Cubic, ...	One depth-factor (H), one breadth-factor (b), and one velocity-factor (say V), ...	V . bH

and are, therefore, liable to error, due to error in estimation of each one of those factors.

But, in computing the several Mean Velocities, the same values of the depth (H) and breadth (b) have invariably been used in the denominators of the expressions ($2a, b, c$) as were taken in computing the original Discharge-Measurements which form the numerators of those expressions. Hence arises a nearly perfect compensation, so that in fact—

"The Mean Velocity-measurements are not sensibly affected by such errors in the Discharge-measurements on which they depend as arise solely from error in estimation of depth or breadth", ... ($3a$).

Thus ultimately—

"The only sensible errors in the Mean Velocity-measurements (U and V) are those in the primary velocity-measurements used in computing the Discharge-measurements themselves", ... ($3b$).

5. **Velocity-Meters.**—Any Instrument used for velocity-measurement may be termed a VELOCITY-METER. Many different kinds have been proposed and tried by various Experimenters: they may be roughly classed as Fixed Instruments and Free Instruments (or Floats).

5a. FIXED INSTRUMENTS.—These are Instruments which being held *more or less fixed in a given position* in the water, measure the current-velocity past them *more or less indirectly.* They are made of many different patterns, but are all open to numerous objections: there is one principal objection, viz., the delicacy of observation required, which necessitates the use of most of them from a *very steady* bridge, the erection of which over a wide channel is of course impracticable. Their use is, therefore, nearly confined to small channels.

[This objection does not apply to Current-Meters, which have been successfully used on many large rivers. The use of these will be discussed in Ch. XXIII.]

5b. Floats.—By this term is meant *any sort* of "floating apparatus" which is dropped into, and abandoned to, the current. For shortness, all such Instruments, of whatever pattern, will be called simply Floats, (whether only slightly or almost wholly submerged.)

All Floats, of whatever pattern, consist essentially of two distinct parts,—

1°. *Submerged parts*, intended to be exposed to the current-action.
2°. *Exposed parts*, intended to serve two purposes.
(*a*), *as a marker*, to render the Float visible from a distance.
(*b*), *as a reserve of buoyancy*, sufficient to bring the Instrument to the surface quickly after any accidental submergence.

The Floats used in these Experiments were of three distinct kinds—

i. Surface-Floats, for measuring surface velocity, (Art. 13.)
ii. Double-Floats, for measuring subsurface velocity, (Ch. IX, 12.)
iii. Loaded Rods, for measuring mean velocity (past a vertical), (Ch. XV, 7.)

6. Essentials of a Float.—The following are the "General Conditions" which should be fulfilled by all Floats, of whatever kind :—

1°. The parts exposed to wind should be the least possible consistent with the function of serving as a "marker", and of possessing sufficient reserve of buoyancy to bring the Instrument quickly to the surface after any accidental submergence.
2°. The submerged parts should be the least possible consistent with their several functions, so as to disturb the natural motion of the water as little as possible.

[In these Experiments the maximum width admitted (of either 1° or 2°) was 3"].

3°. All parts should be of such shape as to expose a nearly constant surface (both directly and laterally) to both wind and current, *however the Instrument turns* during its motion, after attaining its terminal velocity.

[This points to the use of spherical and cylindric shapes in preference to all others].

4°. All parts should be of such materials as to be little affected in their mutual adjustment by alternations of extreme moisture and dryness.

[This points to the use of metal in preference to wood or other hygroscopic materials].

5°. The Instrument should be convenient to handle, and strong enough to bear moderately rough handling.
6°. It should be cheap enough to admit of being made up in large numbers, and light enough to admit of being carried about also in large numbers.

Of these "General Conditions", which should be fulfilled by every Float of whatever kind, Nos. 1°, 2°, 3°, are essential to the requisite delicacy of the Instrument; No. 4° is a practical condition of great importance which is very difficult to fulfil in any hygroscopic material such as wood, cork, pith, &c., along with the conditions 1° and 2° of making all parts as small as possible.

7. Float-Motion.—So many objections have been raised* at times to the use of Floats of any sort on score of inaccuracy, that it seems necessary to discuss the question pretty fully. This occupies the next few Articles.

7a. Forward-Velocity.—On being abandoned to the current, all FLOATS, of whatever kind, move at first irregularly, but acquire after a time a *state of relative equilibrium* with the fluid strata in which they are moving. After this state of relative equilibrium (not necessarily a state of uniform motion) has been attained, the TIME (t) of passage of the FLOAT across the space between two parallel cross-sections of the stream at a known distance (x) apart is carefully timed, and the FORWARD VELOCITY (v_x) of the FLOAT—hereafter called FLOAT-VELOCITY—is *assumed* to be given as the quotient,

$$\left.\begin{matrix}\text{Forward}\\\text{velocity,}\end{matrix}\right\} = \frac{\text{perpend. distance between the cross-sections}}{\text{time of passage between the cross-sections}}, \text{ or } v_x = \frac{x}{t}, \text{.......(4).}$$

This is obviously true so long as the motion of the FLOAT (between the cross-sections in question) is *uniform* and also *parallel to the current-axis.*

7b. *Oblique and Crooked Float-paths.*—It is matter of observation that Floats seldom move really parallel to the current-axis: it may be shown, however, that the above Result (4) is always the required "forward velocity" of the Float, *i.e.*, the *resolved part of its actual velocity* (v) taken parallel to the current-axis in the two following cases:—

1°, when the motion is uniform, and the Float-path straight.

2°, when the forward motion is uniform, even though the Float-path be crooked).

CASE 1°. It is clear that—

$$\left.\begin{matrix}\text{Forward}\\\text{velocity,}\end{matrix}\right\}\begin{cases} = \text{Actual velocity} \times \cos \text{inclin. of float-path to current-axis,} \\ = \dfrac{\text{length of float-path}}{\text{time of passage}} \times \cos \text{inclination as above,} \\ = \dfrac{\text{perpend. distance between the cross-sections}}{\text{time of passage}}, \text{.................(5a),}\end{cases}$$

which proves the proposition.

CASE 2°. Let t_1, t_2, t_3, &c., be the times of passage of the Float through the very short lengths s_1, s_2, s_3, &c., whose projections on the current-axis are x_1, x_2, x_3, &c. Then, as before, the "forward velocities" in the short lengths s_1, s_2, s_3, &c., are $x_1 \div t_1$, $x_2 \div t_2$, &c. Hence since the forward velocity is constant (by hypoth.),

$$\left.\begin{matrix}\text{Forward}\\\text{velocity,}\end{matrix}\right\}\begin{cases} = \dfrac{x_1}{t_1} = \dfrac{x_2}{t_2} = \dfrac{x_3}{t_3} = \text{\&c.} = \dfrac{x_1 + x_2 + x_3 + \text{\&c.}}{t_1 + t_2 + t_3 + \text{\&c.}}, \\ = \dfrac{\text{perpend. distance between the cross-sections}}{\text{time of passage}}, \text{.....................(5b),}\end{cases}$$

which proves the proposition.

7c. Float-Velocity.—In consequence, however, of the Unsteady Motion of the water (as will be explained in Ch. VI, 4), the motion of a Float is seldom continuously uniform and rectilinear (as required in Case 1°), and even its forward motion is seldom continuously uniform (as required in Case 2°), so that the Quotient (4) is certainly not in general strictly equal to the "Forward Velocity" of the FLOAT *at a particular point*, but is really only a sort of—

* *see* Lake River Report of 1869, p. 563 for an Epitome of these.

"Average or Mean of the 'Forward Velocities' of the FLOAT throughout the space between the fixed sections", or, in symbols, $= \left(\int_0^x v_x dx\right) \div x$,......(6).

This quantity—obtained as the quotient (4)—will be called for shortness the FLOAT-VELOCITY, whenever necessary to distinguish it from other velocities.

7d. *Small Floats.*—Next the principle of the use of FLOATS in general depends on the following Hypothesis as to *Small Floats* :—

HYP. The motion of a very small Float—when it has attained the state of relative equilibrium with the fluid—is *assumed* to be the same as the average motion of the fluid particles displaced by it in succession,......................................(7).

Assuming this then, it follows that—

"The Float-velocity may be taken as the measure of the current-velocity",...(8), this last term being understood in a similar sense to the former, viz., not as the Actual Velocity nor even as the Forward Velocity of the fluid *at a particular point* in the channel, but only as a sort of—

"Average or Mean of the 'Forward Velocities' of the fluid particles successively displaced by the Float throughout its path (between the fixed sections)", (9).

A Float does not of course follow the motion of individual fluid particles as they sink or rise, so that the fluid particles displaced—alluded to above—are *different particles in succession at successive points* of the Float-path ; and thus,—

"A Surface-Float measures the average of the forward surface-velocities of the different particles successively displaced by it",......................................(9a).

"A Sub-Float measures the average of the forward subsurface-velocities (in its path) of the different particles successively displaced by it",...... .... (9b).

7e. *Large Floats.*—The above cannot be fairly *assumed* in the case of large Floats. This must in fact remain a subject for special investigation : this question will be taken up for the case of thin loaded Rods (floating nearly vertical) in Chap. XV. The importance of the use of very small Floats will now be understood.

7f. *Quickness of Float-motion.*—It has been urged by some that Floats *always move quicker* than the neighboring water, and this on very different grounds, thus—

Dubuat writes (Principes d'Hydraulique, 1816, Vol. I, Art. 220)—

"A body floating freely on the surface of a uniform current ought to take, and does in fact take, a uniform velocity greater than the central surface velocity * * * * * * When any body whatever floats on a current which has a slope denoted by $1 \div b$, it is situated on an inclined plane, and has consequently a Moving† Force equal to the weight of its displacement of fluid multiplied by the fraction $1 \div b$; that force tends to make it descend, and would accelerate its descent without limit if the body suffered no resistance: then, if we suppose this body moving simply with the velocity of the fluid which surrounds and supports it, it will be at rest relatively to the fluid, and will suffer no resistance from it. Thus its Moving Force will remain intact, and will impress on it repeated increments of velocity, until the excess of its velocity over that of the fluid produces a resistance equal to that force ; then it will continue to move uniformly, and at each instant its Moving Force will be in equilibrium with the resistance

† "force accélératrice"—The context shows that what is called "Moving Force" in Goodwin's Mechanics is here meant.

which it suffers from the fluid. The greater the displacement of water by the body, the greater will be the Moving Force of the body, and the greater also will be the excess of the uniform velocity which it will acquire over that of the fluid ; * * * * *."

Navier reproduces this argument (Bélidór's Architecture Hydraulique, New Ed., 1819, *see* Note on p. 358, Vol. I) in almost similar words.

Both these writers appear to assume that the *terminal velocity* in question will certainly be greater than that of the neighboring fluid : their argument appears, however, only to show that there will be *some terminal velocity* which will *probably be the same as that of the fluid*, for it seems clear that the same argument would apply *equally to any particle of fluid.* In later years the same thing has been urged on new grounds.

Prof. Weisbach writes (Mechanics of Machinery, &c., 1847, Vol. I, Art. 376)—

"As a rule, especially with large and floating bodies, as ships, &c., the velocity of the swimming body is somewhat greater than that of the water : not so much because these bodies in swimming float down an inclined plane formed by the surface of the water, but because they take none, or scarcely any, part in the irregular intimate motion of the water ; still, the variation for small floating bodies is so slight that it may be neglected."

Again, Prof. Jas. Thomson writes—(Royal Socy. Proc. of 12-12-'78, p. 124)—

"On the principle put forward above * * * * a large and heavy boat, even if flat-bottomed and of shallow draught of water, would run down the river-course quicker than the water in which it swims ; for the reason that while all the water surrounding it makes occasional visits to the bottom of the river, and meets with great retardation there, the boat does not dive to the bottom, and is free from any such retardation, * * * * *."

It might be thought at first sight that this would form a great objection to the use of Floats. But this is not the case. For all that is stated is that Floats which are in or near the surface move quicker *on the whole*, than the individual fluid particles which urge them, do *on the whole.* Now, granting this, it remains nevertheless true (as explained in Art. 7d) that—

"Very small Floats do move forwards, *i.e.*, down-stream with the average 'forward velocity' which the fluid particles which come in contact with them from instant to instant have at or about the time of contact", ...(10).

And this is all that is required : for all the Results (5*a*)—(9*b*) above still obtain : so that Surface-Floats measure Surface-velocity, and Subsurface-Floats measure Subsurface-velocity, provided always that they are very small ; whilst the case of large Floats has been reserved for special Examination.

[It is clear that most Velocity-Meters are pretty much in the same position in this respect, viz., that they do not aim at tracing the paths of *individual particles*, but aim at measuring the average of velocities of *successive particles* either—

1°, at definite points, as with most Fixed Instruments.

2°, along definite lines, as with Free Instruments].

8. *Velocity at a point.*—It will be seen that a Float-velocity is not strictly a measure of the forward velocity *at a particular point.* As it is, however, convenient (to avoid wearisome periphrases) to use the fami-

liar phrase "velocity at a point", the following usage will be adopted in this Work:—

"Float-velocities will be held to be velocity-measurements *at points in the Experl. Section, i. e., at the middle points* of the Float-Course (Art. 19),"......(11).

The short phrase "velocity at a point" will accordingly be freely used in this Work, it *being understood to have the above meaning* (when Float-velocities are in question).

9. *Favorable Conditions.*—The conditions that are favorable to the use of Floats are pretty much the same as those laid down (Ch. I, 7) as favorable for Hydraulic Experiments *under simple conditions*, viz.,—

"The site should be situate in a straight uniform Reach of great length, *i. e.*, with uniform Banks, uniform Bed, and uniform Bed-slope for a great distance both above and below the Site", ...(12).

In fact Floats are quite unsuitable for use in cases where the cross-section, bed-slope, or surface-slope change rapidly.

10. *Irregular banks unfavorable.*—Moderate irregularities in the banks do not greatly affect the use of FLOATS at a good distance from them: but—in consequence of the irregular motion induced in the water near them—they render their use difficult at a certain moderate distance, and even impossible pretty close up to them.

But with very long straight uniform banks, Floats can be used pretty close up to the edge: though in this case also their use is difficult very close to, and increases with approach to, the edge.

[At the Soláni Embankment and Soláni Aqueduct Sites, Floats were used—with some difficulty of course—at about 7″ from the edge. Any closer use than this was found practically impossible].

11. **Advantages of Floats.**—When well designed and used under favorable Conditions, the following advantages are claimed for Floats over Fixed Instruments of all kinds:—

1°. In consequence of floating freely in the current they *interfere very slightly with the natural motion* of the current, (far less than any Fixed Instrument.)

2°. They measure the current-velocity *directly*, whereas all Fixed Instruments measure the current-velocity more or less indirectly: indirect measurement is in itself a great source of error.

3°. They can be used in streams of any size, whether large or small, whereas most Fixed Instruments (except perhaps Current-Meters) fail in large or swift streams.

4°. They are not sensibly affected by the presence of either silt or small weeds, whereas Fixed Instruments are liable to injury from silt, and fail altogether in presence of weeds.

5°. They measure the "Forward Velocity", whereas many Fixed Instruments measure (in their ordinary use) either the whole current-velocity, or its whole horizontal part.

6°. They can be made up and repaired by common workmen, whereas Fixed Instruments are extremely delicate, and can only be repaired by professed Instrument makers.

7°. They are very cheap, whereas Fixed Instruments are expensive (£5 to £12 is a common price).

11a. SURFACE-FLOATS, *Advantages*.—As to the advantage of Floats *for surface velocity-measurement*, it will be sufficient to quote the opinion of the International Rhine Measurement Commission, (*see* p. 35,*b* of their Report).

"*Accuracy of Float-Measurements.*—For large rivers Floats are in every way the simplest, surest, and cheapest means for measuring surface-velocities. They are, even when other Instruments and Methods are available, continually resorted to as a means of comparison. For high water of large rivers, the Floats are unquestionably the only means applicable to the observation".

12. Use of Floats in this Work.—For the above reasons FLOATS were *exclusively used* in all the systematic work in the present Experiments. It is not pretended that FLOATS are by any means perfect; one form, the Double-Float, (*see* Ch. IX, 8,) has serious disadvantages: but it is by no means certain that Fixed Instruments have not equally great disadvantages. On the whole, the numerous advantages stated seemed to the author to justify an exclusive reliance on Floats. The justification of the use of the Double-Float will be given in full in Chap. IX, 7.

It will suffice to say here that most of the Sites used in these Experiments were favorable, and some very favorable (even close to the banks) for the use of Floats.

[A few trials of Current-Meters were made in the present Experiments: the experimental difficulties attending their use were not got over, so that scarcely any use has been made of the results obtained].

13. Surface-Floats.—The patterns used on this Work were two—

Pattern i. 3" pine Discs from ⅛" to ¼" thick.
Pattern ii. 1" to 1¼" cork Discs from ⅛" to ¼" thick.

Pattern i was adopted for general use, Pattern ii for use near the margin; except when subsurface velocity-work was being done (Ch. X, 2), in which case the rule was to use Surface-Floats of same pattern as those attached to the Double-Floats.

A small hole drilled through the centre of the Disc permitted of the ready attachment of a pledget of cotton wool to serve as a "marker", when the plain Floats could not be easily seen from the bank.

14. Ropes.—The two Cross-Sections above-mentioned (between which the passage of Floats was to be timed) were always defined in these Experi-

ments by two Ropes tightly strained across the channel perpendicular to the current-axis. These will be termed for shortness the Upper Rope and Lower Rope. The practical arrangements connected with them are detailed in the next few Articles.

[The Ropes used were of various materials according to the span required—
15th Mile Sites, (about 200' span,)—1½" Manilla Rope, from March to May 1878.
" " " " — ¾" Wire Rope, after Novr. 1878.
Soláni Embankment Sites, (178' span,)—1½" Manilla Rope.
Soláni Aqueducts, (82' span,)—1½" Manilla Rope.
Belra, Jaoli and Kamhera Sites, (spans 194', 194'·5, & 170',)—¾" Wire Rope.
Distributaries, (spans under 80',)—Grass Rope].

14a. Low-Level.—In order to note accurately the instant of passage of a Float *under* a Rope, it is clear that the Rope should be *strained at the lowest possible level, i. e.,* as near the water-surface as possible (without actually grazing it), and the Observer should always stand *over the Rope* when noting the passage of a Float under it.

[In these Experiments the two Ropes were accordingly always strained at the *lowest convenient level* consistent with their not grazing the water *when at full supply*: this is indicated on the Cross-Sections of the Experimental Sites (Pl. II, IV, V, VI, &c.) by the upper dotted line in each case. Accuracy would have been gained by lowering the Ropes as the water-level fell, so as at all times to be barely clear of the water, but practical convenience would not admit of this].

15. Ropes, Lifting.—The low-level desirable (Art. 14a) for the Ropes in their working position interfered with the passage of boats, especially at high water. It was in consequence necessary to have the Ropes entirely out of the way of the navigation when not in actual use, and also to have the power of clearing them *rapidly* out of the way in case of boats requiring to pass under them during working hours.

[The power of quick removal was essential, as boats coming down-stream frequently came close to the Upper Rope without being noticed, and would certainly have carried the Ropes away unless quickly lifted].

The lifting arrangements were as follows :—

15a. *Manilla Ropes.*—These Ropes were strained (by hand, no purchase being used) across the Canal daily (care being taken to prevent them getting wet in the process) when required for use, and coiled up again and packed away when the day's work was over. This was of course a great deal of trouble, but at the Sites at which they were used (Soláni Embankment and Soláni Aqueduct) the erection of any permanent gear would have been inconvenient. In case of a boat requiring to pass during working hours, the Ropes were simply slacked off, and raised by hand sufficiently to let the boats pass under.

15b. *Wire Ropes.*—The daily coiling and removal of Wire Ropes would have been almost impracticable, so that a more permanent arrangement was adopted at the four Sites where Wire Ropes were used, (*see* Pl. VII, *Fig.* 1).

The Wire Ropes (*r r*) were kept permanently stretched across the Canal: when not

in use, they were kept lifted high out of the way of traffic by being attached to thin Manilla ropes (r P q), passing over pulleys (P) at the top of "Stand-Posts" (P) erected on the high banks of the Canal. When required for use, the thin Ropes (r P q) were slacked off until the Wire Ropes came down to the water, after which the Wire Ropes were strained taut across the Canal to two stout "Straining-Posts" (S) erected at the lower level (*see* Plate) by means of a single pulley-block (P) at either end. The "spacing" between the Wire-Ropes was maintained correct by passing the Wire-Ropes through notches in the top of "Guide-Posts" (G) erected close to the bank on either side; the distance between the centres of these notches being set out correct.

With this arrangement the Wire-Ropes (r r) could be slacked off from the "Straining-Posts" (S), and lifted high up out of the way (as shown by the dotted line r r) so as to let boats pass under in about half a minute. This arrangement was found to work well, and is as simple as could be wished.

[The Wire-Ropes were found to require great care in coiling and uncoiling: if in any way kinked, one or two strands sometimes broke, and the Rope itself eventually broke when under strain: repairs of a broken Rope proved very expensive and not very satisfactory, the joints being points of weakness. But when sound new Wire-Ropes were used, no difficulty occurred].

16. Pendants.—Pieces of thin white cord were tied on to each Rope at various points, and adjusted (from time to time as the water-level changed) to such a length as to hang freely down and just graze the water-surface at the points at which the Floats were required to pass. From their free *hanging position*, these will be styled PENDANTS.

The Experimental Sites being all of symmetrical cross-section, (*see* Pl. II, IV, V, VI,) the CURRENT-AXIS (Art. 1) was always defined by a pair of PENDANTS placed *over the centre of the Bed*, one on each Rope.

17. Pendant-spacing.—The spacing chosen for the PENDANTS varied with the bed-width and cross-section of each Site for reasons explained in Ch. XVII, 5. The actual positions of the PENDANTS at each Site are shown (by vertical lines) on the Cross-Sections of the Sites, (Pl. II, IV, V, VI, &c).

For spans of under 100′, common 100′ Surveying Chains, supported at frequent intervals from strong ropes or chains (the Surveying Chains not being strong enough to bear their own weight), afford the readiest means of spacing out the PENDANTS *in situ*. But for larger spans the weight of the Chains causes the supporting ropes to sag so much as to be very inconvenient. For such Spans, light Manilla or Wire Ropes, with the whole train of PENDANTS permanently attached, seem to be the most convenient.

To set out the PENDANTS so as to be always in their proper positions when the Ropes are strained taut in their working positions (in spite of the stretching of the Ropes) is not an easy matter in a large span. The arrangements actually adopted were with slight modifications—not affecting the principle—as follows.

17a. *Surveying Chains.*—The arrangement for these was the simplest. The PENDANTS were always fixed on to the Chains at the correct distances (on the Chain) *from the "centre mark"* of the Chain. The Chains were slung from their supporting ropes by wire rings at every 10′, by which they could be slipped along the supporting ropes until the "centre mark" of the chain was vertically over the

centre of the bed. The distances on the chain from the "centre mark" to two points (which will be called for shortness "Chain-marks") vertically over two marks of a permanent nature on either bank (the "Shore-marks")—when the chains were pulled tight—were then noted for future adjustment of the train of Pendants.

17b. *Manilla Ropes.*—The Ropes when first purchased were strained for some days up to their intended working strain, so as to take the slack out of them. They were then stretched across the Canal and strained taut *in their working positions.* Three "Marks" were then made on each Rope, two vertically over a pair of "Marks" of a permanent nature on either bank, and one vertically over the centre of the bed.

[These "Marks" will, for shortness, be called the "Rope-Marks", "Shore-Marks", and "Centre-Mark", respectively].

The Pendant-spaces were sometimes marked out upon the Ropes on shore: they were prepared for marking out by straining them along any convenient level place on shore until the Rope-Marks came flush with two special Marks previously set out at a distance apart equal to the real distance between the two Shore-Marks above. Sometimes they were marked out upon the Ropes from a boat whilst the Ropes were in their "working position" (across the water) as above. Thus the Ropes were always marked when *in their working strain.*

The Pendant-spaces were carefully set out from the "Centre-Mark" with a 10′ Rod: and the PENDANTS themselves were let in between the strands of the Ropes at the places marked.

The spacing of the "Rope-Marks", and the Pendant-spacings were occasionally re-examined.

17c. *Wire Ropes.*—These Ropes not being liable to so much expansion or contraction when in use, a simpler process was adopted. The Ropes were laid out along the ground, and placed under strain in the Workshops when purchased: leaden marks were cast on them, one at the centre, and the rest at the points where the Pendants were to be fastened when in use, the distances being carefully laid out on the ground. When taken into use, they were stretched across the Canal and strained taut *in their working position* with the "centre mark" over the centre of the bed. Two "Marks" were then made on each Rope vertically over two "Marks" of a permanent nature on either bank; the real distance between which was found by theodolite triangulation or otherwise.

[These "Marks" will be called the "Rope-Marks" and "Shore-Marks" as before].

In some cases the space between the two outer leaden marks fixed in the Workshops was found to agree with this distance: in this case the PENDANTS were tied on at the leaden marks (as originally intended). But in several cases the space between the two outer leaden marks did not agree (by several inches) with the distance in question (perhaps in consequence of the sag of the Ropes when *in situ*): in these cases the Pendants were tied on at the correct spacings—as nearly as was possible—*in situ*, the leaden marks being then used only as approximations to the spacings.

18. Ropes, ADJUSTMENT.—From the explanations in Art. 15, it will be seen that each ROPE with its train of Pendants complete had to be adjusted to its correct position every time the Rope was lifted, (*i.e.*, once at the beginning of each day's work,

and also every time the Rope was lifted to let boats pass). The arrangement just explained (Art. 17a, b, c) enabled this to be readily done. The ROPES when placed in their working position were strained until the "Rope-marks" or "Chain-marks" above-mentioned were brought vertically over the corresponding permanent "Shore-marks". The centre Pendants were thus brought vertically over the centre of the bed, and—by the mode of setting out—the whole train of Pendants was thus brought nearly into their correct positions.

[It is not pretended that the spacing thus attained was really accurate: but it was probably as accurate as was practically attainable. The Manilla Ropes for instance occasionally got wet in the process of stretching across the Canal, and contracted considerably for a time, so that it was then impossible to bring the Rope-marks vertically over the permanent Shore-marks (without unduly straining the Ropes). The whole train of Pendants was thus carried a little inwards towards the centre until the Rope expanded in drying. Allowance was made for this by temporarily looping up the Pendants on to the Ropes, so as to hang actually at the correct spacing (ascertained of course by re-setting out temporarily with a 10′ Rod). But it will be seen (Art. 21) that strict accuracy in the Pendant-spacing is not absolutely essential].

The above process for adjustment of the Ropes with their trains of Pendants complete was simple and effectual: it takes, however, so long (about 15 to 30 minutes) in actual execution, that the necessity of raising the Ropes for the passage of a boat proved to be a serious inconvenience in actual work.

[The process of straining the ROPES so as to bring the Rope-marks vertically over the permanent Shore-marks *on both banks* needs of course a little care and time; the vibration caused in the ROPES by the act of straining usually jerked the PENDANTS violently about, and left many of them twisted over the ROPES. This could only be rectified by sending a man out in a *boat* along the whole length of each ROPE to set each Pendant right. Hence arose considerable delay every time the Ropes were lifted. Delays of this sort, of half an hour at a time, are of course extremely annoying under a hot sun].

Practical Remarks.—For Spans under 100′, the use of the Surveying Chains proved the most convenient. For large Spans the Wire Ropes were decidedly the most convenient, being unaffected by accidental wetting, and yielding only slightly under the stress employed in stretching them. The considerable contraction of the Manilla Ropes after accidental wetting, and subsequent expansion on drying, and (to a lesser extent) their yielding under the stress employed in stretching them, are very inconvenient in practice.

19. Float-path, Float-Course.—The *actual* path of a Float in the water will be styled the FLOAT-PATH. The *intended* "Course" of a Float between the Upper and Lower Rope will be styled for shortness the FLOAT-COURSE. Each FLOAT-COURSE was defined by a pair of PENDANTS, one on each ROPE, at equal distances from the CURRENT-AXIS: thus each FLOAT-COURSE was (when the Ropes were in position) set out parallel to the Current-axis.

20. Vertical.—Float-velocities will be accepted (Art. 8) as velocities

at the middle point of the Float-Course as above defined. Conceive a vertical line dropped through that middle point. Subsurface Float-Velocities will be accepted as velocities measured at different points of this vertical line, which may thus be considered as the VERTICAL OF EXPERIMENT, and will be often briefly named simply *the* VERTICAL.

21. Deviation, FAIR COURSE.—From the Unsteady Motion of the water (Art. 7b, c), the actual path of a Float is seldom strictly parallel to the Current-axis; but, as explained in Art. 7b, the value of the FLOAT-VELOCITY obtained is not affected by the inclination of the actual FLOAT-PATH to the proper FLOAT-COURSE; hence the important property—

"A certain amount of DEVIATION from the proper FLOAT-COURSE is admissible", ...(13),

provided of course that the "Deviation" be not so great as to carry the Float into stream-lines of sensibly different velocity.

Now all observation agrees in showing that the velocity-variation in different stream-lines at the same level is very small near the centre, and decreases slowly from the centre towards the banks, near to which it is rapid: from this it follows that—

"The *admissible* DEVIATION from the Float-Course is greatest near the centre, decreases slowly towards the banks, and decreases rapidly for stream-lines very near the banks", ...(14).

Floats—whose DEVIATION from the proper FLOAT-COURSE does not exceed the "admissible deviation"—will be said to be in "fair course", and the Float-velocities resulting will be considered as practically equivalent to velocity-measurements *past one and the same vertical*, or *at one and the same point.*

[The maximum DEVIATION admitted in the present Experiments was—

In streams 150′ wide and upwards—2′ near centre, gradually reduced to 4″ over side-slopes.

In streams 70′ wide and upwards—1′ near centre, gradually reduced to 2″ close to edge.

In streams less than 25′ wide—6″ near centre, gradually reduced to 2″ near banks.

Great attention was paid to the smallness of the DEVIATION admissible in Float-Courses very near the edge, *e.g.*, in the Float-Courses close to (only 7½″ from) the vertical wall of the Solání Aqueduct) where the velocity-variation is most rapid].

For reasons similar to those just given, it will be seen also that—

"Strict accuracy in the position of the PENDANTS defining the Float-Courses is not essential", ...(15).

22. Run.—The space between the two cross-sections in question, or between the Upper and Lower Ropes, being the space through which the passage (or run) of the Floats was to be timed, will be called for shortness the RUN. The question of proper length of RUN will be discussed in Art. 27.

23. Dead Run.—It is necessary (Art. 7a) that the Floats should have attained *a state of relative equilibrium* with the surrounding fluid before entering the RUN within which the time of passage is to be noted. The Floats must, therefore, be cast into the water considerably above the Upper Rope. The space above the Upper Rope necessary for the Floats to acquire this state will be called for shortness the DEAD RUN.

It is desirable to keep this space (the Dead Run) as small as possible consistently with the above essential condition; as the longer this space is, the greater is the chance of large Deviation of the Floats from the proper PENDANT at the Upper Rope, and the greater the waste of time in waiting for Floats which are *in fair course.*

Now all observation shows that—

"The tendency to Deviation is least near the centre, increases slowly towards the banks, and is greatest close to the banks",..(16).

"The tendency to Deviation (near the banks) is much greater with Surface-Floats than with any kind of Subsurface-Floats",..(17).

"Subsurface-Floats take time to attain their state of relative equilibrium increasing with the required depth of submergence",..(18).

It follows that—

"To avoid undue waste of time, the Dead Run must be reduced to a minimum near the banks, especially in the case of Surface-Floats",..(19).

"Subsurface-Floats require increased length of Dead Run as the depth of submergence increases",..(20).

[The actual length of Dead Run employed in the present Experiments was—

For most work not very close to the banks, in wide channels, 100′.

„ „ „ „ „ in narrow channels, (under 30′ wide,) 50′.

For Double-Floats of 7′ depth of immersion and upwards, 100′ to 150′.

For Surface-Floats within a few inches of the bank, 5′ to 20′.

For Double-Floats „ „ „ 20′ to 100′.

For Loaded Rods „ „ „ 50′ to 100′.

In short, the RULE was to use the length of 100′ *in all ordinary cases:* the shorter lengths being used only in narrow channels, or in wide channels only over the side-slopes of the banks, or very close to the banks, so as to avoid undue waste of time, *see* (19) above; and these shorter lengths were always regularly increased—in

work with Subsurface-Floats—as the depth of immersion increased to the *utmost length* compatible with avoiding undue waste of time.

24. Boats.—Two small Boats (of from 15′ to 20′ length) or Pontoon-Rafts (Blanshard's Light Infantry pattern) were in constant use at the principal Sites (exceeding 25′ in width), one above the Upper Rope for "casting" the FLOATS from, and one below the Lower Rope for "catching" the FLOATS after passing through the RUN. These will be styled for shortness the UPPER BOAT and LOWER BOAT. Each Boat was handled and kept in position by a pair of "tow ropes" (usually of thin Manilla cord) held by men on either bank.

24a. UPPER BOAT.—This Boat (from which the FLOATS were "cast" into the water) was placed just above the space above described as the DEAD RUN, and was aligned on the pair of PENDANTS defining the Float-Course, as occasion required. The alignment was corrected by actual trial with a few surface-floats until a position was found from which the Floats could be "cast" so as to enter the RUN close to the proper PENDANT at the Upper Rope. In this way some allowance was made for a side-wind.

[The use of a Boat for "casting" the FLOATS into the water is a necessary evil in a wide channel: the "wash" of the Boat disturbs the water for some distance behind it, and greatly increases the irregularity of motion of Surface-Floats and of Subsurface-Floats only slightly submerged, thus rendering necessary a much greater length of DEAD RUN than would otherwise be necessary in their case. The Rudder was always removed from the Upper Boat so as to reduce the disturbing action of the Boat on the water as much as possible. For work very close to the Banks, the Floats were cast from the banks themselves, and the Boat sent some distance off to get rid of its "wash"].

24b. LOWER BOAT.—This Boat (from which the Floats were "caught" after passing through the RUN) was placed a little below the LOWER ROPE. Its effective use—to enable the Floats to be readily "caught" from it—requires it to be both *pretty steady in the water* (so as not be easily upset by a man's stooping over the edge to catch the passing Floats), and also *handy enough to admit of being rapidly shifted* from side to side by the two tow ropes, so as to bring it within easy reach of the ever-varying positions of the Floats as they passed out of the RUN.

[Any failure to catch a passing FLOAT was always attended with much waste of time, as the Boat had to be sent down-stream to catch it up].

25. Subordinate Staff.—The number of men required for the heavy work for one Field-party depends a good deal on the difficulty of handling the Boats or Pontoon Rafts effectively in a wide channel with a swift stream.

In a moderate stream with a light boat, one man is enough *for each tow rope*: but in a very wide or swift stream two men are sometimes required *for each tow rope*. The distribution of the party was as follows :—

STAFF.	DUTY.	Moderate Stream.	Wide or Swift Stream.
Subordinate Staff, (Natives).	Casting Floats (from Upper Boat), ...	1	1
	Passing Floats (from Upper to Lower Boat), ...	1	1
	Catching Floats (in Lower Boat), ...	2	2
	Handling Upper Boat (two tow ropes), ...	2	4
	Handling Lower Boat (two tow ropes), ...	2	4
	Storekeeper, ...	1	1
	Total, ...	9	13

Thus from 9 to 13 men were required for one Field-party. These men were all natives on a pay of from Rupees 6 to 4 a month.

26. Timing.—The essential Conditions of accurate timing of the duration of visible phenomena, such as the time of passage of a Float over a given space, are—

"The eye should be free to watch the visible phenomena (the passage of the Float under either Rope), whilst the timing should be done wholly by ear",............(21).

"Similar operations at beginning and end of the time should be always done by one and the same Observer, in order to eliminate his personal equation, (in the process of taking the differences of the times counted)",............................(22).

In the present Experiments *all* the timing was invariably done with half-seconds' chronometers with all the care used in astronomical observations, by two *thoroughly trained* OBSERVERS (*see* Ch. II, 4) in the following way:—

26a. *Mode of timing*—One Observer (the "Timekeeper") sat with a field-book and chronometer in front of him *midway between* the two Ropes. The second Observer (who may be styled the "Caller") watched the FLOATS as thrown out from the upper Boat, and warned the Timekeeper of their approach near the Upper Rope; and then—*standing over it*— "called" just as each Float passed under it: he then walked (or ran, if necessary) down to the Lower Rope, and—*standing over it*— "called" again just as each Float passed under it.

The Timekeeper entered the number of chronometer-beats actually counted (from the beginning of his count) just as he caught each "call" to the nearest beat (half-second), or half-beat (quarter-second), according to his skill.

[It will be seen that similar operations at each Rope are throughout done by one and the same OBSERVER, viz.,

1°. *By the "Caller"*—Watching the passage of a Float, and giving an audible signal of its passage;

2°. *By the "Timekeeper"*—Listening for an audible signal, and recording the timing thereof;

also that the distance between the "Caller" and "Timekeeper" was the same at each

"call". As the "time of passage" of the Floats is in each case the *difference* of the number of chronometer-beats counted at each call, most of the effect of "personal equation" of each Observer is obviously eliminated by the above process].

26b. TIMING, PRECISION.—All the chronometer work on these Experiments was done by "trained Observers", (Ch. II, 4). Every Observer on first joining was carefully trained for about a fortnight in the system of "eye-and-ear observing" just explained, and his trial-timings were repeatedly compared with those of the existing trained Staff. No new Observer was passed as a "trained Observer" until the *maximum* Discrepancy between the timings of many successive Floats done by himself and by one of the trained Staff was not more than one chronometer-beat (or half-second).

[On some occasions four Timekeepers have been working together, viz., one old hand, and three beginners, all recording the same Floats: the *maximum* Discrepancies of the timings were found to be the same, viz., one chronometer-beat or half-second].

As the result then of repeated comparisons between the Observers' timings, it may be pretty confidently asserted that—

"The maximum ordinarily possible error* of timing did not exceed one "half-second",..(23).

This precision can only be obtained by use of a clock, chronometer, or watch which beats distinctly (loud enough to be heard, notwithstanding the noise of passing traffic), and not faster than can be readily counted by ear, *i. e.*, not faster than twice or thrice a second. A half-seconds' chronometer (such as is used at sea) suits admirably.

With any *ordinary* watch this precision is impossible: watches usually tick far too fast to be followed by ear, and seldom loud enough to be heard through the noise of passing traffic. It will be seen from the Table on next page that ordinary watches have been employed in all the modern Experiments on large Rivers. This has been a most unfortunate circumstance for the accuracy of those Experiments, for there seems to be no doubt that—

"The maximum ordinarily possible error of timing with a common watch is about two seconds",..(24),

or *four times as large as with a half-seconds' chronometer*.

This has been put beyond a doubt by the International Rhine and Connecticut Experiments, as quoted below.

Rhine Commn., pp. 35, 36.—"The same 25 Floats were seen and called by the same two Observers, while they were recorded by three different Observers with three different seconds-watches." The maximum discrepancies between the timings off the three watches were—

2 sec., once; 1½ sec., once; 1 sec., 7 times; ½ sec., 10 times; nil, 6 times.

* Excluding downright "Mistakes", which cannot of course be estimated.

Connecticut Report of '78, pp. 310, 311. " * * * both observers read the time. * * * * Most of the intervals of time recorded by the two observers agreed; but often the time would differ by one second, and sometimes by two."

EXPERIMENTS.	DATE.	Page of original.	TIMEKEEPER.	LENGTH OF RUN.	CROSS-SECTIONS.
Mississippi, ..	'51-'58	225	Watch, .. [large sec. hand],	200'	2 at 200'.
——[17' Canal], ..	'59	252	Chronometer, ..	51'	2 at 51'.
Lowell, ..	'55	145,148 Pl. XI.	Chronometer & Telegraph, ..	140', 110', & 100',	?
Bazin Expts., ..	'55-'60	145	?	mètres 40 to 50	?
Rhine Commn.,	'67	16, 37	Seconds' Watch,	300',	?
Lake Rivers, .. [1868 Report],	'67-'69	951,952	Chronometer, ..	700', & 1117',	2
Irrawaddi, .. [1875 Report],	'72-'73	16	?	200',	2 at 200'.
Connecticut, .. [1878 Report],	'74	305,310 & 311	Seconds' Watch,	{ 200', & 100' (1 case) }	7 at 50'.
Roorkee, ..	'74-'79	..	Chronometer, ..	{ 50' 25', & 12½', [near banks], }	8 at 25'.

26c. Personal Equation.—Although the effect of "personal equation" is occasionally traceable in these Experiments, still the "Differential Method" of timing adopted (Art. 26a) is such as to reduce its effect to a very small quantity even in any single velocity-measurement. The mode of combination of Sets of Field-work into Series (Ch. VI, 14) is such as to eliminate still further its effect from the Average Velocities which alone are used in most of the Discussions in this Work. It has, therefore, not been thought necessary to endeavor to exhibit its effect in the Detailed Tables (Vol. II) now published in face of the numerous more efficient causes of modification of velocity at work (Ch. I, 13). The means of tracing its effect are, however, given—in case it should be wished to do so—by giving always the Initial of the Timekeeper in each separate Set of Field-work, (*see* Detailed Tables, Vol. II).

[In the 1874-75 Report the attempt was made, see any of the Tables of that Report. But further experience shows that the utility of the additional columns required in each Table is doubtful].

27. Length of "Run".—It is clear that the longer the Run, (or distance between the Ropes,) the less the deduced velocities will be affected by small errors in timing; so that *cæteris paribus* the Run should be

as long as is compatible with the equally important condition that the FLOATS run in tolerably "fair course".

It was soon found in the present Experiments that Floats seldom run for any distance sensibly parallel to the current-axis, but deviate gradually to one side, so that the deviation from "fair course" is *greater with longer Runs;* and greater accuracy of "Course" is attainable with short RUNS than with long ones. A compromise is, therefore, necessary; which while permitting sufficient accuracy of "Course" shall not unduly enhance the effect of small errors of timing. And herein it is clear that—

"Increased precision of timing admits of decreased length of RUN, and, therefore, of increased accuracy of COURSE," .... (25).

[The lengths of RUN adopted, and the timekeeper used in all the great modern Experiments with FLOATS, are shown in the Table on p. 61. It will be seen that a length of not less than 200′ was adopted in the Experiments on large Rivers: and the International Rhine Commission consider (Rhine Commn., p. 87) that—

"To yield trustworthy Results on great Rivers, the longest possible Float-path (of not less than 300′ = 90 mètres), and the greatest possible number of Floats at one and the same point are essentially necessary".

It will be seen, however, that in all these Experiments on large Rivers, *simple seconds-watches* were used, thus entailing large errors in timing, (say about 2 seconds). Now the use of half-seconds chronometers, involving only one half-second as the (maximum) time-error would have permitted the use of a length of RUN of a quarter of above, or say 50′ ($= \frac{1}{4} \times 200'$) *with equal accuracy of timing*, and *greatly increased accuracy of "Course"* of the FLOATS.

It will be seen (same Table) that a Run of only 51′ was used in the Mississippi Experiments along with a chronometer. Also, in the Lowell Experiments (Art. 185) the opinion is given—

"The length may be very short, if suitable arrangements are made for observing the transits, and in rivers of ordinary velocity, a length of 20′ or 30′ would generally be sufficient."]

27a. *Long Runs waste time.*—The question of the proper length of RUN has a further most important practical bearing on Experiment, in that the time occupied in obtaining a given number of good observations increases much faster than the length of RUN itself. It is obvious that the *time of passage* through the RUN increases simply as the length of Run, and that, therefore, the time occupied in obtaining a given number of good observations would also increase simply as the length of RUN, provided that all the Floats moved in equally "fair course" over both short and long RUNS alike: but it was found in these Experiments that the percentage of Floats which fail to run in "fair course", (and are, therefore, unfit to record) is much greater with long Runs than with short Runs, so that to obtain a given number of good observations of Floats all in "fair course" takes up *far more time* with long Runs than with short RUNS; whence it follows that—

"The use of an unnecessarily long RUN leads to great waste of time", ... (26).

It will be proved (in Ch. VI, 5) that in consequence of the Unsteady Motion of the water, all velocity-measurements must be *many times repeated* to be of any practical use. The question of the waste of time attendant on the use of unnecessarily long Runs acquires, therefore, *very great practical importance.* It follows then that—

"The RUN should be the shortest compatible with accuracy in timing,".... (27).

27b. *Advantage of short Runs.*—The advantages claimed for short Runs are then—

1°, Great saving of time ; 2°, Increased accuracy of Course.

28. Length of Run, EXPERIMENTS.—To test the question as to the Length of Run necessary, the following Experiments were devised, and executed at Belra, (for description of Site, *see* Ch. III, 14, & Pl. IV).

Four Ropes were laid out as shown in Pl. VII, 3—

1 at 50', and 1 at 25' above the middle cross-section of the Site,
1 at 25', and 1 at 50' below „ „ „ „ „

so that the spaces between the Ropes were—

25' between Nos. 1 and 2, 50' between Nos. 2 and 3,
25' between Nos. 3 and 4, and 100' between Nos. 1 and 4.

These distances were very carefully laid out with new 10' levelling Staves, along the top of the masonry wall *on both banks* ; the ground was favourable for this being well done on both banks. Two Series of Experiments were tried, one with Surface-Floats in the centre line, one with Loaded Tube-Rods at different parts of the cross-section. Every Float passing in "fair course" was timed as it passed under each one of the four Ropes, in the manner described in Art. 26a.

It is clear that the timing of every "Float" could thus be determined over four different "Runs", viz.,

Upper 25', Middle 50', Lower 25', and Full 100'.

[The Belra Site was not a very favorable Site for the purpose, being a comparatively short artificial channel (only 250' long, *see* Ch. III, 14) in midst of an ordinary earthen channel, and only 743' below a Bridge, so that some difference might naturally be expected between velocities at different parts of its length. Thus the two 25' Runs not being symmetrical about the centre cross-section of the Site might be expected to give somewhat discordant Results].

28a. EXPERIMENT i, (with 1" Tube-Rods, Tab. LXXI).—A set of three of the Rods was timed past each of the 15 Pendants of the Belra Site in the way described in Ch. XVII, 7 as in regular use for Mean Velocity-Curve work : the velocities derived from the means of the three timings of every Rod in "fair course" are given in Tab. LXXI, *q. v.*,

For the 2 Margin-Pendants (marked *m*) over the two 25' and the 50' Runs,
For the 13 Bed-Pendants (for 90' on either side of centre) over all the Runs.

The close agreement of the velocities deduced from the 50' and 100' Runs is quite remarkable, whilst there is considerable discordance between those deduced from the two 25' Runs.

28b. EXPERIMENT ii, (with Surface-Floats at mid-channel, Tab. LXXII).—A

large number of Surface-Floats were run in rapid succession along the mid-channel line. Every Float passing in "fair course" was timed as it passed under each one of the four Ropes above-mentioned, until a total of 48 Floats—all in "fair course"—had been observed. The TIMINGS of each Float over the four different Runs are given in Tab. LXXII in quarter-seconds for the 25′ Runs, in half-seconds for the middle 50′ Run, and in seconds for the 100′ Run, so as to admit of easy comparison of the figures.

The Discrepancies (in the "timings") are shown in the last two lines.

Last line but one. Discrepancy through two 25′ Runs in quarter-seconds.

Last line. Discrepancy through 50′ Run, and half of 100′ Run (in half-seconds).

It will be seen that—

"The Discrepancies between the 50′ and half 100′ Run amount to or exceed 1 half-second in 9 cases; and between the two 25′ Runs amount to or exceed 4 quarter-seconds in 7 cases,"

that is, are much larger between the short (25′) Runs, than between the long (50′ and 100′) Runs.

These differences *between individual* results may seem large; but they are by no means to be ascribed *solely* or even principally to the errors of timing through the different Runs, but for the most part *to the variability of the motion* itself (as will be fully explained in Ch. VI, 4). In fact the discrepancies disappear almost entirely in taking the mean of the 48 Results, as shown below.

DETAIL.	RUN.				DISCREPANCY between	
	Upper 25′	Lower 25′	Middle 50′	Whole 100′	two 25′ Runs.	50′ Run and half 100′ Run.
	Quar. Sec.	Quar. Sec.	Half-Sec.	Half-Sec.	Quar. Sec.	Half-Sec.
Mean of timings of 48 Floats, ...	28·40	29·16	28·44	28·61	·76	·17
Average Velocity,	3·52	3·43	3·52	3·50	·09	·02

29. Standard 50′ Run.—Taking the Results of both Experiments together, it appears that—

"With proper accuracy of timing, the 50′ and 100′ Runs give in general closely accordant Results; whilst the 25′ Runs give Results differing often greatly in detail, though also closely accordant with those of the longer Runs when the means of many trials are compared", ...(28).

The choice of a standard length seems to lie between the 50′ and 100′ Lengths. The advantage (in saving of time, Art. 27a) of the Short Run leads then to the conclusion that—

"With proper accuracy of timing a 50′ Run is to be preferred", ...(29).

[It was found indeed in these Experiments that a RUN of about 50′ is the maximum compatible with obtaining any considerable number of Floats in "fair course"

within a reasonable time in any part of the channel, and that this length must be shortened to avoid undue waste of time) for Float-Courses near the banks where the tendency to deviation from "fair course" is much greater, and where—from the rapid change of velocity in stream-lines at different distances from the bank—any such deviation is of great importance.

The following lengths of RUN were in consequence finally adopted as the STANDARD RUNS throughout these Experiments:—

For general use, ...	50′
Near the banks, *i.e.*, within about 2½′ of vertical or over sloping banks,	25′
For cases of exceptional difficulty, ...	25′
Very near the banks, (*i.e.*, within 9″ of vertical masonry banks), ...	12½′

But it must be observed that the shorter (25′ and 12½′) RUNS were never used, unless the use of the 50′ RUN was found to lead to unreasonable waste of time in endeavoring to obtain a sufficient number of observations of Floats in "fair course".

A good deal depends on the sort of Float in use: thus Surface-Floats are the most irregular in their course, especially if there is any wind; Double-Floats are more regular, and Loaded Rods still more regular in their course. Hence to avoid unreasonable waste of time, it is more necessary to shorten the Run of Surface-Floats than of other kinds. Thus the 12½′ Run was used only for Surface-Floats, (and these only very near the banks,) and the 50′ Run was almost invariably used for the Rods].

30. Laying out Run.—In using such short Runs as 50′, 25′, and 12½′, accuracy in laying out the length in question is of course essential.

[At all the large Sites, the banks were very favorable for accurate laying out. At all these Sites the 50′ space was laid out with a pair of good 10′ Offset-Staves or 10′ Levelling-Staves *on both banks;* the spacing was occasionally re-tested; only trifling differences such as ·01 of a foot were detected.

At the Sites in the small Distributaries, which were not favorable for laying out with 10′ Staves, a new 100′ surveying chain, which had been recently compared with standard Rods at the Government Workshops, was used for marking out both the 50′ and 25′ spaces *on both banks:* great care was used to shake out all kinks in the chain before finally marking out the space.

The shorter spaces of 25′ and 12½′—being required only close to the banks—were usually laid out in the same way on one bank only, whenever required: the space was defined over the water at one end by one of the Ropes of the standard 50′ Run, and at the other end by laying a staff so as to overhang the water].

31. Good Floats, RUNNING FREE, FAIR COURSE.—For shortness' sake the following terms will be used:—

RUNNING FREE. A FLOAT will be said to be *running free* after it has attained the state of relative equilibrium with the surrounding fluid, so long as that state is not disturbed by any extraneous cause.

FAIR COURSE. A Float whose maximum DEVIATION from the FLOAT-COURSE defined by the PENDANTS on the Upper and Lower Ropes does not exceed the "admissible deviation", (Art. 21,) will be said to be in "fair course".

GOOD FLOATS. A FLOAT which was "running free" before its entry into the RUN, and which was both in "fair course" and also "running free" throughout its passage through the RUN will be called a GOOD FLOAT.

[A Float that has been "running free" at one time is liable to be disturbed—so as to be no longer "running free"—by any of the following causes:—

1°, touching the Upper Rope or the Lower Rope, or one of the Pendants.

2°, touching the banks.

3°, touching the bed.

Any one of these accidents must be held to disturb its previous state of running free].

It was a fundamental Rule in these Experiments that—

"No FLOAT which is not a "Good Float" according to the definition above—is to be recorded,"..(30).

[Accordingly no FLOAT would be "called" at the Upper Rope unless likely to prove a GOOD FLOAT: and a FLOAT which had been "called" at the Upper Rope, would still be rejected unless after leaving the RUN it was decided to be a GOOD FLOAT as above. The "Caller" would usually indicate to the "Timekeeper" that a FLOAT whose times at either Rope were already entered in the Field-Book had turned out bad by simply calling out the word "Bad", whereon all entries with that Float would be *immediately erased*].

31a. *Criteria of Good Floats.*—It will be seen that the sole criteria of a FLOAT's being a GOOD FLOAT, (or in other words being *worth recording,*) are that it should be "running free" and also in "fair course" throughout the RUN, without reference to the agreement or disagreement of the time of passage through the RUN with the times of passage of other FLOATS in the same Float Course.

[Great stress is laid on this point: the *practice* of many Canal Officers in India in their ordinary Discharge-measurements is to *select* out of many recorded observations only those of Floats whose times of passage through the RUN were nearly alike. The author believes this system of selection to be *wrong in principle*, all observations made with equal care being entitled to equal confidence. It will be explained in Ch. VI, 4, that the Unsteady Motion of the water *necessarily causes great inequality in the times* of passage of successive FLOATS through the RUN on the same Float-Course: and that it is a prime object to record *all* these times, however unequal, in order to obtain finally a good Average value of the times of passage].

32. **Bad Observations omitted.**—The principle adopted throughout these Experiments (with respect to velocity-work) was to decide in the Field what Observations were "good" and *worth record*, and to record only these permanently in the Field-Book, rejecting and erasing on the spot all Observations considered faulty or doubtful. The Result of this system was that the permanent Field-Book entries were all *entries of good observations*—(all bad ones having been rejected in the field).

[The Field-Book was kept by the "Timekeeper". All entries in the Field were made *in pencil:* the "Timekeeper" was always provided with a piece of "ink-

eraser", with which he erased *on the spot* any entry which from any cause appeared faulty or doubtful].

It was an invariable rule that all Field-Book entries (having been decided to be "good" in the Field) must be accepted as "good", even though apparently unusual or even discordant, unless some obvious "Mistake" could be detected. As far as velocity-observations are concerned, the system adopted enabled *all* obvious mistakes to be detected in the Field, so that *all* velocity-observations passed in the Field as "good" were necessarily accepted afterwards.

33. Field-Book.—A specimen Field-Book page* is shown in Abstract Tab. 33. The "headings" require no explanation.

The "velocity-measurements" at each point were usually repeated three times for reasons explained in Ch. VI, 8, and the mean (of the timings) taken. The Field-Book is ruled into "bands", each to contain the complete data of three successive velocity-measurements at any one point, (or of any one Float-Course.)

In the left column would be entered the "co-ordinate" (*i.e.*, *horizontal distance* from centre, or *depth* below surface) of the point at which the velocity-measurements were to be made.

In the column headed "Times" would be entered the number of chronometer-beats actually counted (from the beginning of the count) as each Float Nos. 1, 2, 3 passed under the two ROPES, viz.,

Upper Rope in sub-column headed "U"; Lower Rope in sub-column headed "L".

The *difference* of the entries in the sub-columns U and L was entered in column headed "D"; this "difference" is of course the number of chronometer-beats (or half-seconds) elapsed during the passage of the several FLOATS 1, 2, 3 through the RUN.

The *mean of these three differences* was next entered in column headed "M. D."; this quantity is of course the average time (in half-seconds) of passage of the three FLOATS through the Run.

All these entries were done *in the Field* in pencil. On arrival in office, all the memoranda at the head of the page, and also the left hand columns and "mean differences" (M. D.) column were inked in, and the "velocities" corresponding to these last were then entered in ink also in this last column: all this work was inked in by the "Timekeeper" (who had made the original entries), *usually on the same day*, (or as soon after as the exigencies of Field-work permitted,) so as to prevent mistakes.

The *whole* of the entries which admit of being checked (*i.e.*, all except those which depend solely on the "Timekeeper", *e.g.*, the entries in the "U" and "L" sub-columns, and the entries of "Gauge-Depth" and "Wind") were then *checked* by the "Caller"; and the Field-Book page signed by both Observers.

[It will be seen that each page of Field-Book admits of entry of two complete SETS of similar work, *e.g.*, two complete SETS of Velocity-measurements on same

* The specimen given is one complete SET of Mean Velocity-work. Some additional explanations are given in the Note on margin of Table.

Vertical, or on same Transversal, with record of Gauge-reading and Wind at beginning and at end of both].

34. Velocity-reduction.—It will be seen (Art. 33) that the whole of the entries in the Field-Book connected with velocity-work, excepting only the final deduction of the velocities themselves from the recorded times, were done *in the Field*.

[The taking out of the "differences" and of the "mean differences" in the "D" and "M. D." columns is not of course strictly Field-work: but the arithmetical work involved is so simple, that it is *easily* done in the Field by a *trained Observer* during the actual progress of the Field-work, and without in any way interfering with the Field-work: and the saving of time (in Office) by doing this (clerical) work in the Field is very great].

The mode of reducing the "velocity" from the "times" in a compendious way is worth attention.

Let n_1', n_2', n_3', be the number of chronometer-beats counted as FLOATS Nos. 1, 2, 3 passed under the Upper Rope, (*i.e.*, the entries in Sub-column "U").

" n_1'', n_2'', n_3'' be the number of chronometer-beats counted as FLOATS Nos. 1, 2, 3 passed under the Lower Rope, (*i.e.*, the entries in Sub-column "L").

" n_1, n_2, n_3 be the number of *half-seconds* elapsed during the passage of FLOATS Nos. 1, 2, 3 through the RUN.

" n = mean of times (in half-seconds) of the three FLOATS within the RUN.

" v = velocity resulting *in feet per second.*

Thus it is clear that—

$$n_1 = n_1'' - n_1', \quad n_2 = n_2'' - n_2', \quad n_3 = n_3'' - n_3',$$
$$n = \tfrac{1}{3}(n_1 + n_2 + n_3), \quad \ldots\ldots \quad (31),$$

and that, therefore—

n_1, n_2, n_3 are the entries actually made in Col. "D" opposite FLOATS Nos. 1, 2, 3,

and, n is the entry actually made in Col. "M. D." in the Field.

Again, (by definition of velocity,)

$$v = \frac{\text{length of Run in feet}}{\text{time elapsed in seconds}}, \text{ being the velocity } \textit{in feet per second.}$$

$$= \frac{2 \times \text{length of Run in feet}}{\text{time elapsed in half-seconds}}$$

$$= \frac{2 \times 50}{n} = 100 \times \frac{1}{n}, \text{ (when the Run was 50')}$$

$$= 100 \times (\text{reciprocal of } n), \ldots\ldots \quad (32).$$

Thus the required velocity could be taken *by simple inspection* out of a Table of Reciprocals, by simply multiplying the Tabular Reciprocal by 100, thus *avoiding all labour of calculation.*

[This saving of calculation is a minor advantage of use of a 50′ Run with a half seconds' chronometer. In the present Experiments the number of velocity-measurements was very large (commonly over 100 daily), so that the saving of calculation was an important matter].

34a. *Result obtained.*—The Result above obtained (32) is not *strictly* a FLOAT-VELOCITY as defined in Art. 4, nor even the mean of the three FLOAT-VELOCITIES of the three FLOATS, for it is clear that the FLOAT-VELOCITIES (v_1, v_2, v_3) of each Float are—

$$v_1 = 100 \div n_1,\quad v_2 = 100 \div n_2,\quad v_3 = 100 \div n_3$$

the mean of which is, $v = 100 \times \frac{1}{3}\left(\frac{1}{n_1} + \frac{1}{n_2} + \frac{1}{n_3}\right)$,.............................. (33),

which is not the same as the Result (32) above.

The Result obtained (32), and used throughout these Experiments may be defined as—

"The FLOAT-VELOCITY of an ideal FLOAT traversing the RUN in the average time of passage of the three real Floats",.. (34).

It may be questionable which Result (32) or (33) is the proper one to use. Custom of other Experimenters has sanctioned the former (32). In the present Work it will be found that single values of the quantity (32) are rarely used (for reasons explained in Ch. VI, 4a) in the discussion of Results, but only *means* of a large number of such quantities.

Now single values of (32) and (33) will be found to differ considerably in consequence of the numbers n_1, n_2, n_3 being commonly very unequal; but the Means of a large number of such Results will *not differ sensibly*, so that as far as the Discussion of Results in these Experiments is concerned, the use of formula (32) in preference to (33) does not in general sensibly affect the Results.

35. Speed of work.—The rapidity of velocity-work with Floats is limited chiefly by the considerable percentage of FLOATS cast from the Upper Boat which eventually turn out not to be "Good Floats" according to the definition given in Art. 31, which causes much unavoidable waste of time, and by the time occupied in exchanging the FLOATS from the Lower Boat to the Upper Boat when the whole available stock have been cast from the Upper Boat. It is also limited partly by the time actually taken by each Float in passing through the Run, and also partly (in case of Subsurface Floats) by the difficulty of lifting them out of the water.

Much time can be saved by the whole of the Staff being trained to work together, and by attention to small details.

One principal means of saving time is by casting several FLOATS from the Upper Boat in such a way as to reach the Upper Rope in rapid succession (at about two or three second intervals) upon the same Float-Course. The chance of some one of the lot turning out to be "Good Floats" is considerable, and there is some chance of several turning out to be "Good Floats".

Should several (say three) *appear* to be "Good Floats" at the Upper Rope, they would all three be "called" in succession at the Upper Rope; and the entries n_1', n_2', n_3', would be made at once in sub-column "U" of the Field-Book. The "Caller" would then *run* quickly down to the Lower Rope (so as to arrive there before the leading FLOAT), and would then "call" again at the Lower Rope for each FLOAT which had been continuously "running free" and in "fair course" throughout the RUN (*i.e.*, the "Good Floats"); upon which the entries n_1'', n_2'', n_3'' would be made in Sub-column "L", supposing that all three were "Good Floats". But supposing any

of them (say No. 2) to be in any way not a "Good Float", the "Caller" would indicate this by simply calling out "No. 2, Bad", and the Timekeeper" would at once erase the entry or entries already made for that Float.

A great deal of time is saved in the long run by adopting the above simple procedure.

[This procedure is applicable only to such Floats (*e.g.*, Surface-Floats and loaded Rods) as can be easily "caught" at the Lower Boat].

36. *Crossing of Floats.*—Such rapid work as above described requires much practice of the whole Staff, but especially on the part of the "Timekeeper". If the succession of FLOATS is very rapid, a difficulty often arises from the Unsteady Motion of the water (Ch. VI, 3d) causing a group of FLOATS which pass under the Upper Rope in the order (say) A, B, C, to pass under the Lower Rope in some other order as B, A, C; or C, A, B, &c.

With trained Observers this should seldom cause any confusion, as the "Caller" should indicate to the "Timekeeper" the order of arrival at the Lower Rope: so that in whatever order they arrive at the Lower Rope, the "Timekeeper" may always enter the counts n_1'', n_2'', n_3'' (Art. 33) opposite the proper FLOAT, viz.,

n_1'' opposite A, n_2'' opposite B, n_3'' opposite C.

Should the succession of Floats be very rapid at the Lower Rope, there is of course some risk of making the entries in the wrong order: but this is of little real importance, for it may be shown that the FLOAT-VELOCITY (deduced as the average of the timings) is *not affected* thereby.

For the "average timing" (n) of the three FLOATS (1, 2, 3) is given by

$$n = \tfrac{1}{3}(n_1 + n_2 + n_3),$$

$$= \tfrac{1}{3}\left\{(n_1'' - n_1') + (n_2'' - n_2') + (n_3'' - n_3')\right\},\text{.........................}(85),$$

$$= \tfrac{1}{3}\left\{(n_1'' + n_2'' + n_3'') - (n_1' + n_2' + n_3')\right\},\text{.........................}(86),$$

and it is obvious from the *form of the last* expression that the value of the final result (n) is not affected by the accident of writing down any of the entries n_1'', n_2'', n_3'' in the wrong order in the Sub-column "L"; although the detailed results n_1, n_2, n_3 will separately be incorrect.

[It is obvious that this Result applies equally to the case of any number (say m) of Floats, provided the velocity be deduced in the same way, viz., *from the mean of the timings*, for it is clear that in this case

$$n = \frac{1}{m}\Sigma_1^{m}(n_m)$$

$$= \frac{1}{m}\cdot\Sigma_1^{m}(n_m'' - n_m') = \frac{1}{m}\cdot\left\{\Sigma_1^{m}(n_m'') - \Sigma_1^{m}(n_m')\right\}\text{..............}(87),$$

which proves the statement].

37. *Precision of velocity-work.*—In the present Experiments the highest velocity ever observed was 7'·1 per second, and a velocity of 5' per second was an unusually high velocity. This last corresponds to a timing of 10 seconds in the standard 50' Run. Admitting that one half-second was the maximum ordinarily possible time-error (Art. 26b) this would amount to $\frac{1}{20}$ of the whole time: from which it follows that—

"Single velocity-measurements of high velocities (5' per sec.) were liable to a *maximum* error of $\frac{1}{20}$ or 5 per cent.", ... (88).

Of course with lower velocities the proportionate error or percentage of errors is much less (because the maximum time-error is unaltered, whilst the total timing through the Run increases with the decrease of velocity): thus—

"Single velocity-measurements of low velocities (1 foot per sec.) were liable to a maximum error of $\frac{1}{100}$ or 1 per cent.", ... (39).

No single velocity-measurements have, however, been used anywhere in this Work, (except for the purpose of exhibiting their irregularity as in Art. 28b and Ch. VI, 3a, b): averages of several (never less than three, and sometimes fifty) velocity-measurements have invariably been used in the Discussion of Results; so that the maximum possible errors in the velocity-data used in the Discussion are *always much less than above.*

The velocities were always taken out to hundredths of feet per second (*i.e.*, to two places of decimals), but the hundredths can only be looked on as *approximately correct.*

[In the Mississippi and Lake River Reports, the velocities are printed to 4 places of decimals (of feet). This gives an *appearance* of accuracy which the author believes to be quite unattainable, and indeed it has since been explained by the Mississippi writers* that the four decimals arose simply from the use of large logarithm tables giving these figures by inspection, and they admit* that these are "evidently more than are needful to represent the observations".

In all the other great modern Experiments, the velocities are shown to only two places of decimals].

* "Reply to Dr. Hagen's Criticisms", Van Nostrand's Maga., Vol. XVIII of '78, pp. 2, 3.

CHAPTER V.

DETAILS.

Preface.—This Chapter treats very fully of certain details connected with velocity-measurement, viz., Water-level (Art. 1—12), Average Depths (Art. 13—17a), Hydraulic Elements (Art. 18—20a), Wind (Art. 21—21d), Miscellanea (Art. 22—26). The Reader who is not interested in the full detail should read only Art. 1, 2, 5—7a, 8—13b, 15, 18, 21—21d, 25.

1. **Water-Level.**—The free surface of a large body of water in motion is generally in a state of slight but rapid oscillation, (and sometimes of a larger slow oscillation in addition to the former,) so that the free surface has *no really definite level.* An AVERAGE FREE WATER-LEVEL only can be found, and that only (with any ease) at the banks.

2. **Still and Free Water-Levels.**—Theory indicates* that—

"The internal pressure in running water (in steady motion) is less than in still water, and decreases with the velocity",..(1);

whence it follows that, in the case of a Still Water Pool or Gauge communicating by a fine tube with a body of running water, the column of the latter above the orifice of communication must be higher than that of the former in order to produce a pressure which shall be equal to (and shall therefore balance) the hydrostatic pressure from the former, or in other words—

"The Free Water-Level is necessarily higher than the Still Water-Level",...(2).

The former Result has been demonstrated experimentally for the case of Pipes (flowing full) by the Experiments† of the late Mr. Froude: the latter Result is confirmed *for the case of Open Channels* by Experiments detailed below (Art. 8); the elevation was found to be *very small.*

[Observe that Result (1) has as yet been proved applicable only *along stream-lines* of fluid in "steady motion". Now it will be shown (Ch. VI, 4) that the motion of large bodies of water is eminently *unsteady:* also the Experiments above-mentioned could not of course be done in "stream-lines"].

3. **Still-Water Gauge,** PERMANENT.—If a large pool of water can be arranged in the bank communicating with the free channel *only by one or more fine passages,* the water-level in the pool will often be found to be *sensibly still,* and will stand of course at a level giving a hydrostatic pressure in the channel of communication equal to the hydraulic pressure in

* Lamb's "Mathematical Theory of the Motion of Fluids", Cambridge '79, Art. 28, 29, 30.
† *See* "Nature", Vol. XIII, of '75, pp. 90, 91.

the free channel at the mouth of the same, and, therefore, presumably a little below the Free Water-Level (Art. 2). A Gauge inserted in such a pool gives the Still Water-Level with great accuracy. This is a very convenient arrangement.

The fine "passages" are liable to become choked with silt, weeds, &c., which would render the Gauge-Readings false: this requires constant attention.

[At three of the Experimental Sites (Belra, Jaolí and Kamhera) permanent Gauges were provided inside still water masonry tanks (or Gauge-Basins, Pl. IV, V, VI), communicating with the free channel by three small passages at different levels. This arrangement was very convenient, and the Readings could be taken with great accuracy. One of the passages was cleared out every day before taking the first reading of the day, to ensure free communication with the Free Channel].

4. Still-Water Gauge, TEMPORARY, (Pl. XXIV, 1, 2).—An attempt was made to secure for the case of Earthen Banks the advantages of a permanent Still-Water Gauge by the following temporary arrangement; consisting essentially of two parts, viz., (1), a Stand-Pipe; (2), a Float-Stick.

4a. STAND PIPE (*Fig.* 1).—A cylindric Pipe (CB) of sheet tin 3" diameter was erected upright in the steep bank with its lower end closed (by a tin disc (B)), and placed at as low a level as could be conveniently reached by digging into the bank, (the water prevented its being sunk more than about 3' below the water-level,) and with its upper end open, and above high water-level. A small lead pipe (*ln*) (of ¼" bore) was soldered into a hole (*l*) in the tin Stand-Pipe about 3" above its foot, (this 3" space (B*l*) at the foot being intended as a Silt-Trap,) and carried out a short distance into the free channel: a length of about 1" at the free end (through which the water entered) was bent vertically downwards and pinched together at the orifice so as to form a contracted nozzle (*n*) pointing downwards; the contraction was intended partly to prevent the entry of weeds, &c., into the Tube, partly to check the oscillation of the water therein. The water stood of course in the Stand-Pipe at a height giving a *hydrostatic* head above the orifice (*n*) sufficient to balance the pressure at that orifice, and, therefore, presumably a little below the level of the water in the free channel, (Art. 2).

4b. FLOAT-STICK (*Fig.* 1).—The only convenient way of determining the water-level inside the Stand-Pipe seemed to be by the use of a Float-Stick (similar to that of a Rain-Gauge), the water being generally too low down within the Pipe to admit of any direct admeasurement.

The Float-Stick (GF) consisted of a slender ⅜" graduated cylindric Stem (G) of a light wood fixed upright in the centre of a hollow cylindric Float (F) of sheet zinc of diameter a little less than that of the Stand-Pipe, so as to float easily in it. The upper end of the Stand-Pipe was fitted with a movable loosely fitting tin cap (C) (of double sheet tin so as to secure some stiffness) with a short tin Tube (*g*) of about ½" diameter by about 1" length fixed (perpendicular to its plane) at its centre. When in use this Cap (C) rested on the top of the Stand-Pipe, and the Stem (G) of the Float-Stick

projected through the short Tube (g), the upper end of which then served as an INDICATOR for taking the readings on the graduated Stem. The Reduced Level of the Indicator (when in position as in use) was found by connecting with the nearest masonry Bench-mark.

The Float-Sticks were graduated *from the top downwards*, so that an increase of the reading on the Stick showed a Rise of water-level. Hence the Reduced Level of the still Water-surface was to be found thus—

$$\text{R. L. of still water-surface} = \begin{cases} \text{R. L. of Indicator} + \text{Reading on Stick} - \\ - \text{constant height of zero of stick above water}, \end{cases} \quad (3).$$

Three of these Float-Sticks—one 3′, one 6′, one 10′ long—were made up for each Stand-Pipe; the shorter ones for use at high water, the longer ones at low water. Being obviously very delicate, they were protected, when not in use, by being placed inside tin tubes (*Fig.* 2) of same length, with a funnel-shaped head to receive the Float (F). Inside these "Protectors" they could be safely carried about. The delicacy of these Float-Sticks is of course an inconvenience, as the fracture of the larger one might render the Instrument temporarily useless.

4c. *Use of the Gauge.*—When about to be used, the first operation was to ascertain if there was free communication between the Stand-Pipe and the Free Channel. A quantity of water was poured into the Stand-Pipe, after which the Float-Stick was introduced, and the Cap placed in position. If the water-level gradually fell within the Stand-Pipe it was judged to be in working order; the lowest reading obtained (when the water had ceased to fall) was taken to be the correct reading.

[Three of these Stand-Pipe Gauges were tried for some time along with the Temporary Free Gauges to be described below, at and near the Fifteenth Mile, Old Site, viz., one at each of the Slope-points (Ch. VII, 2a) 1000′ above and 1000′ below the Site, and one at the Site itself. The Results are detailed in Art. 8. After a while the Stand-Pipe at the lower Slope-Point seemed to get out of order (*see* Art. 8). From the uncertainty attending this, their further use was entirely given up.

The Still Water-Levels (already obtained) were retained only for the Experimental Site itself (*see* Tab., Art. 9), but were entirely rejected for both Slope-points (*see* Tab., Ch. VII, 3), except indeed for the sole purpose of contrasting with the Free Water-Level, Art. 8].

4d. *Conclusions.*—From the experience gained, it would seem that the Instrument in the rough form above described cannot be depended on in a stream bearing silt, weeds, &c., from the liability both of becoming clogged, and of separating at the joint (l) (a point of weakness), which might destroy the free communication with the water. But if made and erected in a more permanent fashion, so that the constancy of level of the "Indicator" can be depended on, and if efficient means of clearing the communication with the free channel be provided, there can be no doubt that the Instrument would have all the advantages of a good Still Water-Gauge.

5. Free Water-Level.—In the absence of a Still Water-Gauge, the Free Water-Level must be determined. Its oscillations will be found too rapid to admit of observing with certainty—even in calm weather—any but the highest and lowest water-levels. Even these levels are variable, there being frequent phases of high maxima and low minima.

In a high wind the uncertainty is much increased.

The practice of these Experiments was to watch the water-surface (with the eye as close to the water-surface as was practically possible) for *about half a minute*, and record the "highest maximum and lowest minimum" within that time. It was then *assumed* that—

AVERAGE FREE WATER-LEVEL = Mean of "highest maximum" and "lowest minimum", ... (4),

and this mean level is the one that was *always used for the Free Water-Level* in recording Gauge-Readings, &c., in these Experiments, whenever the highest and lowest levels themselves are not shown.

6. STANDING FREE GAUGE.—A permanent Standing Gauge is the only convenient way of thus determining the Free Water-Level; but to admit of very accurate reading, it is essential that it *should not ruffle the water-surface*. This can be arranged in a long straight pretty uniform Bank of masonry, or wood, by building the Gauge into, and *flush with*, the bank, so that the water may slip quietly past without being ruffled; or in an irregular bank (as in an earthen channel) by placing the Gauge in an indentation in the bank.

7. **Temporary Free Gauge**, (Pl. XXIV, 3, 4).—In these Experiments it was necessary to determine the water-level frequently at many places where no Standing Gauge existed, and where it would have been difficult and expensive to erect one. The following Arrangement, which may for shortness be called a TEMPORARY FREE GAUGE, was found to give very accurate determinations of Free Water-Level, and to be convenient in application.

7a. *Temporary Gauge*, (*Fig.* 3, 4).—The essential feature is the provision of a number of permanent or temporary Bench-marks in the bank, the lowest of which shall be below low water mark, and the rest ranged at convenient levels, so that there may always be found one *at a depth less than one foot* below the temporary water-surface. It remains then only to measure the depth of water over this last Bench-mark.

Brass Foot-Rule, (B). This was done by inserting a thin brass Foot-Rule (B), made of sheet brass 1" wide by $\frac{1}{16}$" thick, divided into tenths and hundredths of a foot, into the water *with its thin edge to the stream*, and *with its foot placed upon the Bench-mark* in question: this thin Rule was found to ruffle the water-surface only very slightly, so that it was possible to read the "highest maximum" and "lowest minimum" water-level *with great accuracy*, to hundredths or even half hundredths of feet: (the length of this Rule (one foot) determined the Maximum difference of level admissible between successive Bench-marks).

[It is absolutely necessary to the success of this process that the Bench-marks should be so arranged that the water-surface is not ruffled in its motion past them].

Bench-Marks. The detailed arrangements for the Series of permanent or temporary Bench-marks mentioned were as follows :—

7b. *Solánі Embankment* (Pl. XXIV, 3).—The erection of a Standing Gauge anywhere within this length (2¾ miles) would have been difficult and expensive. The flights of 12 masonry steps, each about 9" high, (Pl. II, 2,) afforded, however, the means of establishing a series of *permanent Bench-marks* at about 9" intervals, at all places where the water-level was to be frequently determined.

For determining the water-level when it fell below the lowest step, a "referring mark" was cut in the face of the 4′ "drop-wall" (Pl. II, 2), and the distance of the water-surface from this mark measured with a Rod or Rule.

[This last measurement was not susceptible of so much accuracy as in the former case, especially if the distance to be measured exceeded one foot, from the impossibility of bringing the eye near the water-surface].

7c. *Earthen Banks*, (Pl. XXIV, 4).—Temporary Bench-marks were provided by driving a series of stout wooden Pegs into the bank at convenient intervals of about 9" or 10" as far as they could be readily driven with a mallet; the heads were then sawn off roughly horizontal. A brass headed nail *with a round head* (n) was then driven home on the top of each Peg: the rounded top afforded a *definite level* (not easily mistaken) upon each Peg to serve as "temporary Bench-mark".

[In some cases (at the Slope-Points connected with the Belra and Kamhera Sites) this series of Pegs was retired in a pool formed in an indentation in the bank, and partially cut off from the free channel by a rough earthen Dam, so as to form a nearly still water pool].

A masonry Pillar was built on the bank near to the series of Pegs to serve as a "permanent referring mark": the levels of the "temporary Bench-Marks" were checked by connecting them (by levelling) with this referring mark as often as was thought necessary, sometimes daily, sometimes weekly.

7d. *Inconvenience of temporary arrangements.*—The constancy of level of the "temporary Bench-marks" cannot be depended on in soft soil, nor even in firm soil when freshly driven; and they are liable to be tampered with by passers by. The necessity of frequently checking their levels is of course a good deal of trouble in extra Field-work.

The temporary arrangements described give also some trouble in office in reduction of the Field-records to a common datum, all of which is saved by the use of a Standing Gauge.

8. Free higher than Still Water-Level.—This Result, rendered probable by the arguments in Art. 2, is confirmed by the following Experiments undertaken chiefly to test the use of the Stand-Pipe Gauge.

Experiment. Three of the Stand-Pipes above described were *erected alongside* of three of the Temporary (Peg) Gauges above described in use at and near the Fifteenth Mile, Old Site, on the Left Bank, viz.,

One of each at the Slope-Point (Ch. VII, 2a), 1000′ above the Experimental Site.
One of each at the Experimental Site.
One of each at the Slope-Point (Ch. VII, 2a), 1000′ below the Experimental Site.

The Still and Free Water-Levels of the same place were determined *in succession* (*i.e.*, not simultaneously) by the same Observer from the Stand-Pipe and Peg respectively, in connexion with the Discharge-Measurements detailed in Tab. XLIX. in progress at the Experimental Site. The Results reduced to a common Datum (870′ above Karáchí Mean Sea Level) are shown in Tab. LXXV.

[These Experiments would have been better for this purpose if the Still and Free Water-Levels of the same place could have been determined *simultaneously:* unfortunately this was impossible with the Staff available consistently with the proper execution of the other more important work in hand. The actual intervals of time between the Still and Free Water-Level observations was about a minute or two on each occasion].

It will be seen (*see* Table) that out of 63 (*i.e.*, 21 + 24 + 18) trials, the Free Water-Level is *slightly higher* than the Still Water-Level in 54 trials, coincides with it in 3 trials, and is the lower in only 6 trials: moreover, these last 6 depressions occur at one place, on only 3 days in all, and are all comparatively large, so that it seems quite probable that the Stand-Pipe concerned was *out of order* on those days.

From the above it may be fairly concluded that—

"The Free Water-Level is in general higher than the Still Water-Level by a small quantity",..(5),

whence follow the important practical Conclusions—

"When great accuracy is required, the Free Water-Level should be taken", (6*a*).

"For ordinary practical purposes, it matters little whether the Free or Still Water-Level be taken, but it is desirable to adhere constantly to one or the other at the same spot",..(6*b*).

9. *Water-Levels at each Site.*—The different modes of determining the water-level at the different Experimental Sites, are shown in the Abstract below.

Reach.	Site.	Bank of reference.	Description of Gauge.	Remarks.
Roorkee Reach.	15th Mile Sites, Old,	Left	Temporary Still-water Gauge, Art. 4,	Very accurate.
	15th Mile Sites, New,	Left	Temporary Free Gauge, Art. 7c,	
	Solání Sites. Embankment, [All Sites],	Left	Temporary Free Gauge, Art. 7b,	Very accurate on steps. Fairly accurate below steps.
	Solání Sites. Left Aqueduct, ..	Right	Permanent Free Gauge, [marked in deep notches on stone slab let flush into face of central pier].	Fairly accurate, [ruffle the water slightly].
	Solání Sites. Right Aqueduct, ..	Left	Permanent Free Gauge, [marked on enamelled iron Bar nailed on face of central pier, projecting ⅜" into stream].	
Lower Reaches.	Belra, ..	Right	Permanent Still-water Gauge, [fixed in Still-water Gauge-Basin].	Very accurate.
	Jaolí, ..	Left		
	Kamhera, ..	Left		
Distributaries.	Right Jaolí, ..	Right	Permt. Free Gauge [marked on enamelled iron Bar nailed on masonry pillar]. 100′ below Site,	Fairly accurate, [ruffle the water slightly].
	Mansúrpur, ..	Right	50′ below Site,	
	Pimora, ..	Right	148′ below Site,	
	Míránpur, ..	Left	Temporary Free Gauge, Art. 7c,	Very accurate.

10. Gauge-Reading.—For shortness' sake the height of water-level above any convenient datum will be styled a GAUGE-READING, whether obtained as an actual reading of a permanent Gauge, or by any of the temporary arrangements above described.

11. *Water-Levels, Both banks.*—In determining the water-level at a Site by observation on one bank, the question naturally arises whether the Free Water-Level is the same at opposite points on either bank in calm weather, and whether it is affected by wind.

Experiments. To test this point, a series of careful Experiments were made on four different days, viz., on two days in a perfect calm, and on two other days in a high wind across the canal, (one from right to left bank, and one from left to right bank,) all at the Solání Embankment Main Site, which is very favorable for accurate determination of the free water-level, (Art. 9).

To obtain the highest possible precision, the Average Free Water-Levels (Art. 5) at the opposite banks were in every case determined *as nearly as possible simultaneously* by two Observers acting in concert, thus—

"The two Observers knelt down at the water's edge on opposite banks at the same time, and noted (by the mode described in Art. 7) the highest maximum and lowest minimum water-level which occurred at their respective stations *within about the same half-minute.* The Mean of these readings was accepted (Art 5) as the Average Free Water-Level on either bank: and the difference of water-levels at the two banks found by applying the known reduced levels of the steps. Each such observation was repeated several times in succession".

The details of these Experiments are given in Tab. LXXVI, LXXVIII.

An Abstract of the Results is also given in Table below: the following abbreviations are used—

R > L stands for Right Surface higher than Left.

L > R stands for Left Surface higher than Right.

Reference No.	DATE.	Central Depth.	WIND. From D.	V.	To D.	V.	Banks sheltered from wind.	exposed to wind.	State of Canal.	Number of trials.	Max. Oscillations. Left Bank.	Right Bank.	DIFFERENCE OF AVERAGE FREE WATER-LEVELS at the edges. from	to	Maximum Divergence.	Mean.
1	19-5-'77	10'·4	...	0	...	0	...	...	Rising	12	?	?	·008, R > L	·004, R > L	·004	·005, R > L
2	23-6-'77	11'·2	...	0	...	0	...	...	Rising	24	·06	·04	·015, L > R	·029, R > L	·044	·000
3	19-8-'78	8'·5	W	17	W	18	R	L	Rising	6	·08	·05	·015, L > R	·040, L > R	·025	·030, L > R
4	30-5-'79	11'·3	E	27	E	30	L	R	Falling?	10	·20	·30	·055, L > R	·070, R > L	·125	·014, R > L

The state of the Wind was observed at beginning and end of each Series of Experiments: the mode of recording it is explained in Art. 21.

From the above Abstract the following Conclusions may be drawn :—

"Even in calm weather, the Average Free Water-Levels of opposite banks are liable to differ slightly at any one time, (probably on account of the varying state of the oscillations,)" ....(7a),

"but are sensibly the same on the average of a considerable interval",......(7b).

"In a high cross wind the oscillations are greater than in a calm, and increase with the wind", ...(7c).

"In a high cross wind the oscillation is least under the sheltered bank",...(7d),

"and the Average Free Water-Levels of opposite banks are liable to differ at any one time by a good many hundredths of a foot, and irregularly",..........(7e),

"but on the average of a considerable interval the Average Free Water-Level is *slightly raised at the bank which is exposed* to the wind",.......................(7f).

[It may seem a matter of surprise that (if the Wind does really raise the Average Free Water-Level on the whole on the bank exposed to it) the mean elevation so produced by the very high wind in the fourth Experiment is actually much less (·014 as compared with ·030) than that produced by the more moderate wind of Experiment No. 3. But the fact is that the difficulty of the observation increases rapidly with the height of the wind on account of the violence and irregularity of the oscillations caused, so that the last Experiment is by no means so trustworthy as the third : the uncertainty is well shown by the magnitude of the "Divergence" (·125) of the Results].

As it appears thus that the Average Free Water-Level is liable to differ considerably on opposite banks (and also at other parts* of the surface) at any one moment, it seems desirable to adopt for the Average Free Water-Level at a Site some quantity independent of the particular bank of observation : the only obvious simple (*i. e.*, easily obtained) measure thereof seems to be the following :—

"The Average Free Water-Level at a Site = Mean of the (simultaneous) Average Free Water-Levels on opposite banks",..(8).

With this definition, the following practical Conclusions may be drawn from the above Experiments :—

"In calm weather and also in a moderate wind the Average Free Water-Level at a Site may be determined with sufficient accuracy for most purposes by a single complete observation (*i. e.*, of both max. and min. oscillation) on one bank",...(9a),

"and in calm weather with great accuracy by several complete observations,"(9b).

"Observations would be necessary on both banks in a very high cross wind to secure even moderate accuracy, and also in a moderate wind to secure great accuracy",.. (9c).

12. Mean Water-Level.—It frequently happens that the WATER-LEVEL in question changes somewhat during the course of an Experiment. To meet this difficulty, the practice in these Experiments was to observe and record

* *See* Basin Expts., Atlas, Pl. XIX—XXIII, & XXVI, (wherein many cross-sections of water-surface are figured ;) or *See* Ch. VIII, 2a of this Work for brief abstract of same.

in the Field-Book the Average "Gauge-Reading" *at the beginning* and *again at the end* of each SET of Experiments of any one kind: and it was then *assumed* that for all purposes of calculation or discussion—

"MEAN WATER-LEVEL (of the Experiment) = Mean of initial and final Water-Levels", ...(10*a*),

and similarly—

"Mean Gauge-Reading (of the Experiment) = Mean of initial and final Gauge-Readings", ...10*b*).

Entry in Tables. This last quantity—the MEAN GAUGE-READING—is what is entered in Col. 2 of most of the Detailed Tables as the "Argument", or quantity showing the state of the water-level. The variation of water-level (or difference between initial and final Gauge-Readings) is also given alongside to show the state of steadiness or unsteadiness of the water.

[In the sub-column "Variation" (of water-level) throughout the Tables, a RISE of water-level is indicated by the sign +, and a FALL by the sign −].

13. Average Cross-Sections.—In the case of Sites with uneven beds, it would seem always necessary to obtain a sort of *Average Cross-Section*, by taking Soundings right across the channel at several cross-sections a short distance apart, so as to eliminate the effect of casual irregularities of the bed. This seems especially necessary when the Results are to be used in connexion with Float-velocities, which are necessarily only a sort of Average velocity along the Float-Course. For this case the AVERAGE DEPTH along the Float-Course, or perhaps even throughout a longer line, should be obtained.

SOUNDING COURSE. This term will be used for shortness to denote a line of Soundings parallel to the Current-axis.

[The number of cross-sections sounded over in all the large modern Experiments with FLOATS, from which the Average Cross-Sections were obtained, is shown in Tab., Ch. IV, 26b. It will be seen that only two Cross-sections were sounded over in most of the large Rivers. This seems certainly far too few for the purpose].

13a. *Practice on this Work.*—At all the Experimental Sites with uneven beds, Average Cross-Sections of the bed were obtained by taking Soundings right across a number of (from 6 to 8) cross-sections at 25′ to 50′ apart ranged above and below the centre of the Site (as in Sketch, Pl. VII, 2), viz.,

8 Cross-Sections at 50′ apart in all the wide channels.
6 „ „ „ 25′ „ in the Distributaries.

The Soundings were taken in Sounding-Courses aligned with (and extending beyond) the Float-Courses, (defined by the lines of Pendants,) which lay over the bed. The mean of the depths in each Sounding-Course was accepted as the AVERAGE SOUNDING in that Course, and is the only Result of the kind used throughout the Work.

Thus the AVERAGE SOUNDING is the average of from 6 to 8 Soundings covering a space of 125′ to 200′ (along the Sounding-Course) which includes both the "Dead Run", the "Run" itself, and a short space below the "Run".

Regular Banks. Over permanent masonry banks, and also over regular earth slopes, no Soundings were taken, as the Cross-section (of such portions) was more accurately obtainable by measurement. These cases comprise—

Earth Slopes, At the 15th Mile New Site, (Pl. II, 1.)

Masonry Steps, At the Solání Embankment Sites, (Pl. II, 2.) The levels of Treads of Steps were found by levelling.

Masonry Slopes, At the Belra and Jaolí Sites, (Pl. IV, 3, & V, 3.)

13b. *Frequency of sounding.*—The bed and banks of the Ganges Canal are—wherever not artificially protected—liable to constant irregular change, by erosion at high water when the current is strong, and by partial silting at low water when the current is slack: and even where artificially protected, the bed is liable to partial silting at the period of very low and slack water which occurs *just before the closure* of the Canal for repairs, &c., and also *just after the re-opening* (after repairs), soon after which the Silt deposit is swept away by the current. It was accordingly necessary to determine at frequent intervals the Average Cross-Sections at all the Experimental Sites in Earthen Channels. This was of course not equally necessary in the Solání Embankment, the bed of which is protected with frequent brick and boulder bars, (Ch. III, 9,) or in the masonry channels of the Solání Aqueduct.

The actual dates of Soundings are shown below.

REACH.	SITE.	CHANNEL.		SEASON of Experiment.	DATES of Soundings.	REFERENCE.		
		Banks.	Bed.			Text.	Table.	Plate.
Roorkee Reach.	15th Mile Sites, Old,	Earth	Earth	March to May '78	28-3-'78, 31-5-'78	III, 8	I	II, 1
	15th Mile Sites, New,	"	"	Novr. & Dec. '78 April '79	16-12-'78 28-4-'79			
	Solání Embankment, Main Site,	Masonry	Clay, brick & boulder bars	Aug. '76 to Dec. '78 April '79	15-8-'76, 4-6-'78 28-9-'78, 13-11-'78 & 16-12-'78	III, 10	I	II, 2
	Solání Embankment, Minor Sites,	ditto.	ditto.	Jany. '75	Jany. '75	III, 11	..	XXVIII
	Solání Twin Aqueducts,	Masonry	Masonry	Dec. '74 to April '79	Occasionally	III, 12	..	II, 4 I, 3
Lower Reaches.	Belra, ..	Masonry	Earth	Jany. to March '79	Weekly	III, 14	II	IV
	Jaolí, ..	"	"	Jany. to March '79	"	III, 15	III	V
	Kamhera, ..	Earth	"	Jany. to March '79	"	III, 16	IV	VI
	4 Distributaries,	"	"	March '79	As required	III, 17	LVI	XLI

14. CROSS-SECTIONS, TABLES and DIAGRAMS.—The Results are shown in Tables and Plates, somewhat differently for the wide and narrow channels.

14a. *Wide Channels*, (Tab. I to IV, & Pl. II, IV, V, VI, XXVIII).—The Results, viz., the AVERAGES in each Float-Course, obtained from the Soundings, *reduced in every case to a common datum* for each Site, are shown (in *old face* figures 4·32) in

Col. 3 of Tab. I to IV, each line of which may therefore be held to show an AVERAGE CROSS-SECTION of the Site. Underneath each "Average Height of Bed above datum" is shown (in *old brevier* figures, as 2·9) the RANGE of the depths along the Float-Course concerned, *i.e.*,

"The difference between the max. and min. Soundings along the Float-Course".

These figures will give an idea of the *roughness of the bed*. Thus it will be seen that the Soundings vary frequently by 1½′ along one Float-Course.

The (actual) Central Depth (in general nearly the greatest depth) is also given in Col. 2 for each day of Soundings. This will give an idea of the difficulty of taking the Soundings on each occasion.

The last line of each Table shows (in italic figures) the RANGE of the AVERAGE HEIGHTS along each Float-Course *throughout the whole season*, *i. e.*,

"The difference between the maximum and minimum Average Heights along the Float-Course",

and may be held therefore to show roughly the amount of change (erosion or silting) that took place in the season.

Diagrams of one or more of the typical Cross-Sections of each Site are given in Pl. II, IV, V, VI, and XXVIII, on a common scale of 25 feet to an inch. These Cross-Sections are also repeated at foot of all Plates of Transverse Velocity-Curves (Pl. XXVI to XL).

14b. *Distributaries*, (Tab. LVI, & Pl. XLI.)—In consequence of the small number of Experiments in the Distributaries, it was found more convenient to print the Results in the form of AVERAGE SOUNDINGS (or Average Depths below the actual water-level), and in the same Table (LVI) as the velocity-measurements in connection with which the Soundings were taken: they are shown in Col. 6 of Tab. LVI in the lines marked "Depths", with their "Ranges" (or difference between greatest and least Sounding obtained) printed underneath. Each such line, therefore, shows an Average Cross-Section of the Site.

The Cross-Section Diagrams are given on Pl. XLI on a scale of 10 feet to an inch.

15. Average Depths.—The MEAN WATER-LEVEL having been determined (Art. 12) for every Set of Experiments, the AVERAGE DEPTH along any Float-Course in any particular SET of Experiments was found as follows:—

Level Bed, (Solání Aqueduct). The Average Depths were assumed to be the same as the Average Gauge-Reading, the Gauge-Zero being on the floor of the Site.

Uneven Bed. The Average Depths were found by applying the *change of water-level* since the day of sounding (say Δh) with its proper algebraic sign to the known Average Sounding (h).

Thus in all cases the estimation of Average Depths depends ultimately on the *correct determination of water-level*, and is therefore difficult on a windy day: hence (*see* Art. 11)—

"The Average Depths are liable to over- or under-estimation (by a few hundredths) in a wind blowing across the stream according as the Water-Level is taken on the lee or sheltered shore", ...(11).

Effect on Discharge-measurement.—As the depth enters *directly* as a factor into most* computed Discharges (both Discharges past a vertical, and Cubic Discharges), this source of error can by no means be overlooked.

16. Sounding Rod, (Pl. VII, 4).—This was a wooden Rod from 11' to 15' long of 1½" × 1½" square section, marked in feet and tenths of feet—in such a manner that the subdivisions were easily recognizable at 100' distances as follows, (*see Fig.*):—

Opposite faces of the Sounding Rod were exactly alike.

On any one face (and on the opposite face), every alternate foot only was subdivided into tenths, each tenth being painted alternately all black or all white; and the intermediate feet were painted either all black or all white alternately.

On adjacent faces the subdivision and painting was quite similar, but the monochrome and graduated feet were alternated.

The Rod was fitted with a sheet-iron "shoe" about 4½" diameter to prevent its sinking into the silt or into accidental (small) holes between bricks, boulders, &c., on the bed; this shoe was loaded with sufficient lead to carry the Rod quickly to the bed. The head of the Rod was fitted with an iron ring to which a light rope was attached to save the entire loss of the Rod in case of its slipping out of the hands of the man who wielded it, (as sometimes happened.)

17. Mode of Sounding, (Wide Channels).—The Soundings were taken from a boat with the Sounding-Rod just described: the reading being done from shore.

The two ROPES being placed in their "working positions" with their train of Pendants complete, (Ch. IV, 18,) defined two of the Cross-sections (*see* Pl. VII, 2), on which soundings were to be taken: the remaining six cross-sections were defined by marks laid out on the banks.

The Boat was then aligned in any one of the lines of Pendants some distance above the upper section; and when all was ready, allowed to float gently down the stream. One of the native Staff let the Sounding Rod into the water and kept lifting it up a few inches (or as much as was necessary to let its foot escape the irregularities of the bed) and letting it go, whilst the Boat floated gently down-stream, holding the Rod as upright as possible. In this way of sounding—with the Boat in motion—the disturbance of the water round the Sounding Rod was reduced to a minimum. The alignment of the Boat in the line of Pendants was preserved by tow-ropes handled by men on either bank, who walked along the bank with the Boat as it floated down-stream. The Junior Observer sat in the Boat and directed the alignment by *aligning the Sounding Rod* itself with the line of Pendants. The Senior Observer walked along the nearest bank, watching where the water cut the Sounding Rod: and noted the reading just as the motion of the Boat carried it into the cross-section desired; the proper moment was indicated by one of the native Staff standing on the Bank in the plane of the cross-section, who called out just as the Sounding Rod crossed that plane.

17a. *Precision of Soundings.*—Soundings made as described, *i.e.*, with a Sounding Rod read from the banks, cannot be taken even under favorable circumstances (*i.e.*, in a narrow channel and in slack water) much closer than the tenth of a foot.

* Not into Discharges past a Transversal, (*e.g.*, Surface-, Mid-depth-, &c., Discharges.)

In these Experiments the Sounding Rod was always read *only to the nearest tenth of a foot:* occasionally this could not be done with certainty. But great accuracy in reading the Sounding Rod would have been quite useless on account of the unevenness of the bed, the accidental rises and hollows in which were often 1 foot, and sometimes more, (*see* Tab. I—IV.)

The AVERAGE SOUNDINGS obtained—as the mean of six to eight soundings 25′ to 50′ apart in each line of Pendants—can alone be accepted as approximate AVERAGE SOUNDINGS *generally correct to the nearest tenth* of a foot; but accuracy in the hundredths cannot be depended on.

[Greater precision in the AVERAGE SOUNDINGS could only have been obtained by increasing the number of cross-sections sounded over: but the labor of obtaining soundings over a larger number was quite prohibitory. The work described, viz., sounding over eight cross-sections in from 15 to 19 Sounding-Courses usually occupying 4 hours of work fatiguing to the whole party].

18. Hydraulic Elements, COMPUTATION.—It is proposed to apply the term HYDRAULIC ELEMENTS to the following quantities which are of frequent occurrence in Hydraulics:—

Gauge Reading (h or H), Central Depth (H), Surface-breadth (b),
Wet-Border (B), Area (A), Hydraulic Mean Depth (R).

These quantities vary generally with the water-level, defined by the Gauge-Reading (h or H), and require therefore special determination for any particular water-level.

It seems necessary to explain here the mode of computation employed.

Computation of H, b, B, R. These call for no remark.

Computation of A. The formulæ used were in all cases the best simple approximation-formulæ (Simson's Rule, Cubic Rule, Weddle's Rule, &c.) known: they will be found fully detailed in the Chapter on Discharge-computation (Ch. XIX): the formulæ being of same type in both cases, it has been found most convenient to give them in detail once for all* in that Chapter. It will suffice to say here that it is by no means a matter of indifference what formulæ are used, for it will be shown (Ch. XIX) that, in channels which are as a whole *concave throughout*, the use of simpler formulæ than those used in this Work (*e.g.*, the Trapezoidal Rule) *tends to under-estimation* of the Area.

The labor of computation—especially in the case of the channels with changing beds—is very great. Isolated values must of course be computed *directly;* and this work admits of no abridgment except such as can be had from methodical computation on ruled forms. But where many values are required, *e.g.*, in forming a Table, it suffices to compute directly a number of fundamental values at moderately close intervals, and also *at every point of discontinuity* in the cross-section, (such as the treads of steps, Pl. II, 2,) and to interpolate the remainder.

18a. *Earthen Channels.*—In the case of Earthen Channels, the values of H, b, B,

* *See* Abstract Tab. 11 for detail of formulæ used at each Site.

A, were computed directly, *i.e.*, from the Average Depths only for the gauge-readings (h) of each day *on which Soundings were taken* (Art. 18b): these were the fundamental values. For other water-levels, the Bed was assumed constant, and the *changes* of H, b, B, A corresponding to the changes of water-level (Δh) were computed, and applied as "corrections" to the fundamental values of H, b, B, A. The changes in question can usually be computed much more readily than new values of the primary quantities, especially when there are permanent banks of masonry at uniform slope.

For very small changes of water-level (such as ·01 or ·02), the value of R was often obtained by simply assuming its "change" to be *equal to the change* of water-level. But for larger changes of water-level, the value of R was always obtained by direct division as the quotient B ÷ A, the "change" of R not admitting of ready computation.

18b. *Side-Slopes of* 1 *in* 2.—The above process is particularly easy of application in the case of Side-slopes of 1 in 2 (as at the Belra and Jaolí Sites). For it is clear that these Side-slopes are such that—

$$\text{Rise : Base : Slope} = 2 : 1 : \sqrt{5} = 1 : \tfrac{1}{2} : 1{\cdot}118, \quad\ldots\ldots\ldots (12).$$

Hence the true surface-breadth (b_0) having been found (once for all) for any given gauge-reading (h), it is clear that for any given change of water-level (Δh),

$$\text{Change of water-surface } (\Delta b) = \text{change of water-level } (\Delta h), \ldots\ldots\ldots (13).$$

Also the true Wet-Border (B) and Area (A) having been computed from the Soundings for any given gauge-reading (h), it is clear that for any given change of water-level (Δh),

$$\text{Change of Wet-Border } (\Delta B) = 2{\cdot}236 \times \text{change of water-level} = 2{\cdot}236 \times \Delta h, (14).$$

$$\text{Change of Area } (\Delta A) = \tfrac{1}{2}(b_0 + b) \times \text{change of water-level},$$

$$= \tfrac{1}{2}(b_0 + b) \times \Delta h = (b_0 + \tfrac{1}{2}\Delta h) \times \Delta h, \ldots\ldots\ldots (15),$$

where b_0 = water-surface of day of Soundings,

$$b = \text{water-surface of time of Expert.} = b_0 + \Delta b_0 = b_0 + \Delta h, \ldots\ldots (16).$$

[Similar formulæ might of course be readily constructed for any other side-slopes].

19. DETAIL PUBLISHED.—To save the labor of such calculation hereafter, their values are given either once for all in special Tables, or else in such detail as seemed requisite in the Detailed Tables of velocity-work.

Earthen Channels. The bed being liable to change, the values of each of the quantities (H, b, B, A, R) have been given *in detail with each day's work*, (Tab. XLIX to LVI) computed from the Average Cross-Sections given in Tab. I to IV, and LVI.

Solání Embankment Minor Sites, (Surface velocity-work only). The values of H, b are given *in detail with each day's work*, (Tab. XXXIII). The work being of comparatively little importance, it was considered unnecessary to compute the values of B, A, R: they can be computed from the Average Cross-Section figured in Pl. XXVIII.

Solání Embankment Main Site and *Aqueduct Sites.* On account of the assumed permanence of the bed at these Sites (Ch. III, 10a, 12a) it has been possible to present the values of the Hydraulic Elements in short special Tables (Nos. V, VI) for ready reference. These will be explained below. In addition to this, however, the values of the more important elements (H, b, R) have been given *in detail with each day's work* in the case of all Transverse Velocity-Curve work, (Tab. XXIX to XXXII,

and XXXIV to XLVIII,) so that for this sort of work reference to the special Tables is required only for the values of B, A.

19a. *Soláni Embankment Main Site.*—Two special Tables (*see* Tab. V) have been prepared, one for the whole of the Two-Year Period (August '76 to August '78), in which the bed was found nearly constant, and one for the subsequent period with variable bed.

The latter shows the values of each of the quantities H, *b*, B, A, R, for the actual water-level (defined by *h*) of every Experiment made in this latter period, the Experiments being few in number.

The former shows the values of each of the quantities H, *b*, B, A, R throughout the whole range of water-level as follows:—

1°, *Below the Steps.* At or near the water-level of every actual Experiment.

2°, *Above the Steps.* At the level of the Tread of every Step, at ·01 above every Tread, and at every tenth of a foot.

An easy interpolation gives the values at any intermediate level.

This (Two-Year) Table was computed from the Average Cross-Section of the bed obtained by soundings on 15-8-'76 (Tab. I), together with the cross-section of the steps obtained by levelling done about same time. The former (August '78 to April '79) was computed for each day from the soundings most recently executed (Tab. I), together with the cross-section of the steps obtained by levelling in September '78.

[It should be noted that the steps are of somewhat unequal height, and the treads of corresponding steps on either bank are—probably in consequence of unequal settlement—on different levels, (the left bank steps being about ·15 the higher:) the steps average $1\frac{1}{8}'$ tread with ·75' rise; their levels may be seen in Tab. V. These inequalities, and the abrupt change of figure at each step, have involved very full detail in Tab. V].

19b. *Soláni Aqueducts.*—The special Tab. (No. VI) has been prepared from the enlarged Cross-Section of the Right Aqueduct given in, Pl. I, 3, and shows the values of each of the quantities *b*, B, A, R as follows:—

1°, *Below 4'.* At or near every gauge-reading (H) at which Experiment was done.

2°, *Above 4'.* At every tenth of a foot of gauge (H).

3°, At every point of discontinuity of the contour; these correspond to *h* or H = 2'·00, 7'·30, 9'·37, 9'·85, (*see* Pl. I, 3.)

For gauge-readings intermediate to these, the values can be found by an easy interpolation. The Table applies in strictness only to the Right Aqueduct, but has been used for the Left Aqueduct; the Aqueducts are so nearly similar that the error involved must be small.

20. Hydraulic Elements, Discontinuity.—It is obvious that, as a general rule, the values of each of the quantities H, *b*, B, A, R, increase and decrease with rise and fall of water-level: but this is by no means universal. In the first place in earthen channels with beds liable to change, every re-determination of the cross-section figure is liable to cause a change in the values of each of these quantities even for the *same gauge-reading*, and to quite destroy any regularity in their increase and decrease. This will explain sufficiently the irregularities noticeable in the case of the earthen channels (Tab. XLIX to LVI).

Next in permanent channels, (and also in changing channels between each new

determination of the cross-section,) the changes of these quantities are of different character, viz.,

1°. The values of H, B, A always increase and decrease with rise and fall of water-level.

2°. The value of *b* usually increases and decreases with rise and fall of water-level; but between vertical banks it is constant, and in the exceptional case of overhanging banks its change is the opposite of the change of water-level, (as in the Soláni Aqueducts, Pl. II, 4.)

3°. The value of R usually increases and decreases with rise and fall of water level; but at every abrupt discontinuity, *e.g.*, at the tread of every step, (Pl. II, 2,) and at every horizontal offset (as in the corbelled footways of Pl. II, 4), the value of R usually *decreases at first* with any very small rise of water-level, and then increases as the water-level continues to rise. This is of course a consequence of the value of R being the quotient A ÷ B; the value of B just above such points of discontinuity *increasing abruptly* (by the width of a whole step or offset), whilst that of A increases at first very slowly.

[*See* Tab. V, VI for many instances of this].

20a. Hydraulic Elements, *Discrepancies.*—It seems necessary to explain here that the Hydraulic Elements for the Soláni Embankment Main Site figured in the Tables and Plates, differ *in a few instances* somewhat from those of the Standard Tab. V. These discrepancies are due to trifling differences (·01 or ·02) in the levels of the treads of the steps as determined at different times. In all such cases the detailed values are to be preferred as depending on the standard levels of the time.

[The differences in question are liable to cause discrepancies of like amount (·01 or ·02) in the values of H, R; no discrepancy arises in the value of *b*, except when the water-surface is nearly flush with the tread of a step, in which case they are liable to cause discrepancies of the breadth (1·2′) of a whole step in the value of *b*, and of about ·05 in that of R, (*see* Tab. XLIV, Ser. 161 for several instances; this case is quite exceptional)].

21. Wind.—The Direction and Velocity of the Wind were recorded usually both *at beginning* and again *at end* of each SET of velocity-work of any one kind, as follows:—

Direction. This was estimated in the following conventional manner (very convenient, however, for the purpose), viz., by the usual mariner's compass points, *i.e.*, as N., N. *b* E., N.N.E., &c., the current-axis being taken as WORKING MERIDIAN, or N.S. line, thus—

A Wind blowing *from up-stream* is reckoned *North.*

" " *from down-stream* is reckoned *South.*

Sometimes the Wind appeared to be in no definite direction; this is denoted by the letter V, which stands for "Variable".

Velocity. This was measured by observing the *time* occupied in one *revolution* of the primary index of a small Anemometer. The number of chronometer beats (half-seconds) elapsed were alone entered in the Field-Book. The published results show in all cases the deduced Velocity *in feet per second.*

In a few cases special letters have been used to indicate particular states of wind not readily shown by the Anemometers, thus—

l stands for *light*, *i.e.*, too light to move the Anemometer.

g stands for *gusts*, indicating *squally*,

and in a few other cases (when there was *no Anemometer available*) the following special letters have been used—

l for light, *m* for moderate, *b* for breeze, *h* for high, *g* for gusts (or squalls).

21a. Anemometers.—The Instruments available were—

A. Air-Meter (Biram's pattern) by Negretti and Zambra, showing 150′ of wind per revolution.

B. Air-Meter by Casella, showing 100′ of wind per revolution of primary index.

C. Anemometer (Robinson's pattern), by Adie, showing 180′ of wind per revolution of primary index.

These Instruments were used as follows:—

Site.	A	B	C
15th Mile Sites, ...	March to May '78,	Decr. '78 & April '79,	... ...
Solání Embankment,	1876—'79 (except as under B)	March to May '78,	... ...
Solání Aqueducts, ...	'75 to Novr. '78,	... ...	Decr. '78 & April '79
Belra Site, ...	Jany. to March '79,	... ...	... ...
Jaolí Site, ...	... ...	Jany. to March '79,	... ...
Kambera Site, ...	... ...	... ...	Jany. to March '79

Thus it will be seen that Instrument A was used almost continuously for the two principal Sites (Solání Embankment, and Aqueduct): and in particular this Instrument was used during the whole of the Subsurface Velocity-work, (with trifling exceptions.)

[It is unfortunate that these Instruments were of different patterns, especially as their indications were found not to agree* very well, when tested together: but there were no others available].

21b. *Wind-data rough.*—The Wind data obtained can only be looked on as a *rough* Estimate of the wind, partly in consequence of the data giving the values of the direction and velocity *only at beginning and at end* of each complete Set of velocity-measurements of one kind, disregarding therefore the change (sometimes very great) of wind during the progress of the Field-work of the Set, (lasting say ½ hour to 4 hours,) and partly in consequence of the uncertainty of the indications of the Instruments themselves.

[It would have been preferable *perhaps* to have found the Total Wind passed during the whole time occupied in the Field-work of each complete Set of velocity-mea-

* The Superintendent of the Thomason College Meteorological Observatory, Dr. Murray Thomson, F.R.S.E., reports that he has found this to be a common fault of Anemometers, even when of same pattern.

surements of one kind. The only Instrument (A) available from 1875 to 1878 (i.e., during most of the Field-work) was, however, quite unsuited for such work].

Inasmuch, however, as there is as yet no known way of making any quantitative allowance for the effect of the wind, and so eliminating it from the actual Float-velocities, it is believed that the Results obtained are sufficient for the present purpose as below—

"The Wind Results are to be accepted only as rough indications of the presence and amount of some cause of disturbance of the motion of the water",.........(17).

21c. Mean Wind.—The MEAN WIND of several observations of direction and velocity of wind has been found as follows :—

The RESULTANT WIND was first found by plotting to scale the whole of the wind-data ("Calms" and "Light" Winds being reckoned as of zero velocity) by the Theorem of the POLYGON OF FORCES (or of velocities).

[In finding this Resultant, only such data could be employed as gave *definite values* of both the *direction* and *velocity* of the wind, to the entire exclusion of all indefinite estimates, such as "direction variable", or such as "high", "gusts", &c., for velocity].

The *direction* of the MEAN WIND was then taken the same as that of the RESULTANT just found, and the *magnitude* of the MEAN WIND was found by dividing the magnitude of the RESULTANT by the number of wind-data, (excluding of course all not employed in finding the Resultant.)

The entries of Mean Wind in this Work have all been found by the above process. It will probably be admitted to be the most correct value of the Mean Wind obtainable from the available data.

[Where a number of velocity-measurements have been made in various states of the wind, the Mean Wind seems to be the natural and proper quantity to present along with the Means of the velocities, (because the resultant effect on the motion of the water itself may be expected to be roughly as this "Mean Wind".) But it seems questionable whether certain other Wind-Results, *e.g.*, the Total Wind and Maximum Wind do not also require attention at same time. Thus, in the process of forming the Mean, Winds in opposite directions cancel each other more or less, so that it often happens that the Resultant and Mean Wind of a SERIES containing many high (but more or less opposing) Winds is but small, whilst the Total Wind would be large.

[This Result may be seen in very many of the Detailed Tables VII to LXX].

Now the difficulty of the Experimental work increases a good deal with the Wind, so that the "weight" or reliability of any Series may be said to be *pro tanto* inversely as the Total Wind (divided of course by the number of Wind entries).

21d. Wind, DIAGRAMS, (Pl. XXI, XXII, XLIV—XLIX).—The Wind (amount and direction) is shown in the Plates by firm arrows (———→ or ———|) plotted from the "Wind Zero Lines" at the foot of the Plate. The length of the arrow shows the magnitude of the Wind, and the direction of the arrow shows the direction of the Wind, (the arrow-head always *pointing towards the wind's eye.*) The "Wind Zero Lines" being East and West lines, East and West Winds could not well be plotted on them : all East and West Winds have, therefore, been displaced a little above or

below these lines for sake of distinctness. In a few other cases, it has been found necessary for distinctness to displace the indicator-arrow slightly from the usual position (with its root on the "Wind Zero Line"): this should, however, cause no difficulty in examining the Plates, as the root of each wind-arrow will always be found either *on the ordinate* to which it belongs, or connected with that ordinate by suitable guiding lines.

Calms and Light Winds are indicated by *black dots* on the "Wind Zero Line".

A separate Wind Zero Line is used for each sort of Field-work for which different Winds are recorded, thus—

Pl. XXI, XXII, XLIV contain only one Wind Zero Line.

Pl. XLV to XLVIII contain three Wind Zero Lines, (corresponding to the Mean Velocity-, Central Surface Velocity-, and Surface Slope-Results).

Pl. XLIX contains two Wind Zero Lines (corresponding to the Mean Velocity-, and Central Surface Velocity-Results).

Again, on Pl. XLIX the Wind is shown twice, *i.e.*, for both beginning and end of each separate SET both of Mean Velocity- and Central Surface Velocity-work by arrows with flat and sharp heads (———| and ———>) respectively. Thus two arrows are plotted for each ordinate from each "Wind Zero Line": with winds in different directions, each arrow has its root on or near the Zero Line; when both winds are in the same direction, the second arrow (———>) starts from the head of the first, (thus ———|———> = ———| + ———>).

In the other Plates, only one Wind (viz., the Mean Wind of the Series) had to be shown for each Result.

22. Reduced Levels.—The whole of the Reduced Levels given in this Work are referred to the Great Trigonometrical Survey of India Datum (Mean Sea Level at Karáchí): those determined by the Experiments' Staff having been invariably connected with at least two of the Canal Bench-marks for this purpose.

The Reduced Levels of comparatively unimportant points, (*i. e.*, unimportant to this work) *e. g.*, of Floors of Bridges and Falls, Crests of Falls, &c., have been accepted from the Canal Records without further verification. The Reduced Levels of the whole of the Gauge-Zeros—except that of Chitaura Falls Gauge—were verified by the Experiments' Staff by connexion with the two nearest Canal Bench-marks.

[The Canal Bench-marks are nearly all on the plinths of milestones, or on the plinths of certain pillars in the wing-walls of the bridges. Most of these are *plastered*, and are otherwise unsuitable for really good permanent Bench-marks. The plaster is in many cases worn away, in some cases the milestones have sunk, and in some cases seem to have been moved (?) since the time when they were recorded as Bench-marks. The Reduced Levels of the Canal Records are, therefore, *frequently inaccurate*. For this reason all *important levelling*, (such as required for surface-slope* measurement, &c.,) was in all cases done independently. Its mere connexion with the Canal Bench-marks in no way affects the results].

All special levelling done by the Experiments' Staff was always done twice over

* For the detail of the delicate levelling required for this, *see* Ch. VII, 4.

with an excellent 20″ Level, and repeated until all Discrepanices exceeding ·01 of a foot were cleared up.

23. Precision in Results.—The *primary* data used in calculating Results in these Experiments are chiefly Reduced Levels, Gauge-Readings, Average Depths, Breadths of Channel, and Float-velocities. These were all taken out (originally) to two places of decimals, (*i.e.*, hundredths of feet,) and sometimes (in case of reduced levels) to three places.

All calculations from these data were usually taken out to two places of decimals (*e.g.*, hundredths of feet,) and sometimes more: many of the Results are of course not accurate to two places of decimals, and it has not been thought worth while in such cases to retain so many figures in the published Results. The number of decimals observed, used, and retained in each quantity are shown below.

Detail.	Depths.						Breadths			Velocities.		Area.	Discharges.				Abstract Quantities.	
													Superficial		Cubic			
	Reduced Levels.	Gauge-Readings.	Soundings.	Average Depths.	Hyd. Mean Depths	Surface-Falls.	Surface.	Bed.	Wet Border.	Float.	Average.	Area.	past a vertical.	past a transversal.	large (over 1000 c. ft.)	small (under 1000 c. ft.)	Surface-Slope.	Co-efficients.
Observed, ..	2 or 3	2 or 3	1	..	..	2	2	..	..	..	..	..	..	..	..	..	..	..
Computed, ..	2 or 3	2 or 3	2	2	2	2	2	2	2	2	2	2	2	2	2	2	6	2 or 3
Published, ..	2	2	2	2	2	2	1	1	1	2	2	1	1	1	0	1	6	2 or 3

It will be seen that the principle adopted as to retention of decimals in the *published* Results was to retain only—

1°, 2 decimals in all linear measurements, such as depths, and velocities, which seldom exceed 10′ or 12′.

2°, 1 decimal in all considerable lengths, such as breadths.

3°, 1 decimal in all superficial measurements, such as areas, and superficial Discharges.

4°, 1 decimal in small volumes, such as Discharges under 1000 c. ft., and none in large volumes, such as Discharges over 1000 c. ft.

It is submitted that the number of decimals published is in each case quite as many as is useful.

[*Ex.* The impossibility of expecting accuracy even in the units in the larger Cubic Discharges will be obvious from the consideration of even a single source of error; thus, an error of only ·01 of a foot in determining the Average Water-Level at a Site 150′ wide with a mean velocity of 4′·5 per sec. causes an error of $\cdot01 \times 150 \times 4\cdot5 = 6\cdot75$ c. ft. per sec. in the computed Cubic Discharge, besides which there are many other sources of error].

24. Means, *Discrepancies in.*—Two sorts of *apparent* slight Discrepancies are liable to occur in the "Means" in the printed Tables; these are not really "Errors", but are discrepancies inherent in the process of forming the Means.

Discrepancy 1°. The "Means" being usually the Arithmetic Means of the detailed entries in the several Sub-Columns, it may happen that the means of such quantities as are obtained by computation (such as b, B, A, R, D, U, **D**, **V**, &c.) will frequently *not agree exactly* with the values which would be obtained by direct computation from the Means of the data. And exact agreement is of course usually impossible* in such cases.

Discrepancy 2°. Again, the printed "Means" being the Means of the *original* details (including usually two places of decimals) are not always the exact Means of the *printed* details in cases where decimals have been rejected in printing (*e.g.*, in the Breadth, Area, and Discharge Columns). Such discrepancies are always small.

In all these cases the printed Means are the ones to be preferred.

25. Checking.—It was made a fundamental Rule of these Experiments that *all work* (such as copying, computing, drawing, &c.,) *which admits of check*, should invariably be *checked by an independent hand*, and *initialed by both parties*. Similarly all "corrections" were checked, and initialed by two parties.

Thus all the Field-books are supported throughout by the Observer's and Checker's initials at foot of each page. Similarly all MS. Tables and Computation Sheets are supported by the Copyist's or Computer's, and also by the Checker's initials at foot. Also every addition, alteration or correction is similarly supported. In the same way every original drawing is initialed by the original† Draughtsman and by the ‡Checker. All these original MS. could, therefore, be re-examined at any future time, if found necessary, with some confidence.

Similarly all Tables and all matters of fact in the Text of this Work have been checked by one‡ of the Staff.

The printing of the Tables and Plates was checked in the way described in the Introductions to Vols. II, III. The revision of the Text in passing through the Press was done by the author himself.

26. *Aids to calculation.*—It has already been explained that—in consequence of the use of a 50′ Run with a half-second's chronometer—most of the velocity-reduction could be done by simple inspection of a Table of Reciprocals.

Much of the multiplication involved in the subsequent Discharge-computation consisted of forming numerous pairs of products of "velocity × depth" (seldom exceeding 3 figures in each factor (*e.g.*, $v = 4{\cdot}93$ *ft. per sec*, $h = 8{\cdot}35$, and $vh = 4{\cdot}93 \times 8{\cdot}35$). All such products are given at once *by inspection* in Crelle's large Multiplication Table. Similarly quotients of 6-figure numbers by 3-figure numbers are also given *by inspection*. The heavier multiplications and divisions of figures beyond the powers of Crelle's Tables, were usually done upon an Arithmometer.

[Each Field-party was provided with a copy of Barlow's Tables of Squares and Reciprocals, and of Crelle's Multiplication Table: and two Arithmometers were kept in the general computing office. These proved invaluable].

* This should be obvious from the consideration that the arithmetic mean $\frac{1}{n}\Sigma f(x)$ of n similar functions $f(x)$ differs usually from the similar function $f\left(\frac{1}{n}\Sigma x\right)$ of the mean $\frac{1}{n}\Sigma x$ of the data (x).

† The author himself. ‡ Nearly all by the Senior Checker, Sergt. W. Porters.

CHAPTER VI.

UNSTEADY MOTION.

1. Steady and Unsteady Motion.—It is well to premise the following definitions* :—

DEF. The motion of a fluid is said to be STEADY when the velocity at each point depends *solely on the position of the point*, and does not vary from instant to instant; and is said to be UNSTEADY when the velocity at each point *varies from instant to instant*,.. (1).

Thus in STEADY MOTION the velocity at each point is a function of the co-ordinates (x, y, z) of the point, but not of the time; and in UNSTEADY MOTION the velocity is a function both of the co-ordinates and of the time. These cases are expressed analytically thus—

Steady Motion, $v = f\ (x,\ y,\ z),\ \ \dfrac{dv}{dt} = 0$,.. (2).

Unsteady Motion, $v = f(x, y, z, t),\ \ \dfrac{dv}{dt}$ not zero,.................................. (3).

Examples. Water in uniform motion in a fine straight tube of great length is in "Steady Motion". Water in waves, ripples, or eddies is in "Unsteady Motion".

The mathematical investigations of cases of UNSTEADY MOTION are—except in the simplest cases of waves and whirls in a frictionless fluid—very difficult. Those of STEADY MOTION are far easier. Accordingly most of the common hydraulic formulæ are *based on the hypothesis that the motion of water is steady.* This is, however, a hypothesis at complete variance with observation. This result, viz., that the motion of water is extremely "unsteady" is of such great importance in forming a rational Theory of Hydraulics, and in its effect on practical Experiment, that it has been thought worth a special Chapter.

2. Preliminary Experiment.—Any one watching the motion of water near the margin in a large channel *apparently in tranquil motion* cannot fail to remark a continuous *slight variation* of level of very short period. This shows that the motion even when apparently tranquil is slightly oscillatory, and, therefore, not quite "Steady".

[On the 2½-mile length of masonry steps of the Soláni Embankment this oscillation is sometimes about *half an inch*, and runs through all its phases very rapidly].

* *see* any Work on Mathematical Theory of Motion of Fluids, (*e.g.*, Lamb's Work, Art. 28).

Again, any one throwing a handful of chips of wood or other light material into water *in apparently tranquil motion* in a large channel cannot fail to remark *great irregularities* in their motion. Some will lag behind for a time, then suddenly hurry on and catch up others previously in advance, pass them, and perhaps after a time lag behind again. Few will move in straight lines; most of them will take devious courses crossing sometimes to right, sometimes to left. This shows unmistakeably that the motion is decidedly "Unsteady".

Those who have not seen velocity-measurements being performed, can, however, hardly be aware of the magnitude and rapidity of the variation of the velocity at one and the same place. It is in fact so great as to prove a source of extreme difficulty in Experiment.

3. Unsteady Motion, EXPERIMENTS.—This fact (the magnitude and rapidity of the variation of velocity at one and the same place) is clearly shown in Tab. LXXIII, LXXIV.

3a. CENTRAL SURFACE VELOCITIES.—Tab. LXXIII shows the results of 17 SETS of central surface velocity-measurements (48 in each Set), selected from a very large number of such Sets of 48 repeated central surface velocity-measurements at each Site, (done in connexion with Discharge-measurements to be hereafter explained;) the selection is such as to exhibit the phenomenon (of variability of the motion) under a great many widely different circumstances, and masked as little as possible by extraneous accidental influences (such as wind). Thus the work of calm days only has been selected, and at both the highest and lowest available water-levels at each of eight very different Sites, (the largest being 192′ wide, and the smallest 13¾′ wide.)

All the velocity-measurements (of this Table) were done with same Instrument (8″ pine Disc), and in same manner, and in every case only GOOD FLOATS (as defined in Ch. IV, 31) were recorded.

The 48 velocity-measurements were in every case done as rapidly as possible in succession, so that the whole Set of 48 was in each case done in the shortest time possible (in one case in only 15 minutes) consistent with recording only Floats in "fair course". These Sets are, therefore, very favorable for showing the variability of the motion.

3b. CENTRAL VELOCITIES.—Tab. LXXIV shows the results of 10 Sets of velocity-measurements upon the centre vertical, viz., 2 Sets at the surface, 7 Sets at various depths (5′, 6′, 9′) below the surface, and one Set of mean velocities. The subsurface velocities were measured with five different Instruments, viz.,—

DOUBLE FLOATS, 4 patterns, *see* Ch. IX, 2a, 11, 12.

CURRENT-METER, Moore's pattern, (*see* Procgs. of Inst. of Civ. Engrs., Vol. XLV, No. 1481.)

The Experiments shown in this Table were all "comparative trials" of two or more Instruments. They are not so favorable for exhibiting the phenomenon now under review (Unsteady Motion) as those of the preceding Table; partly in consequence of the existence of wind (on three of the days reported), which is itself a cause of *some* irregularity even in subsurface velocities, and partly in consequence of the Experiments being "comparative trials" of Instruments, which led to the velocity-measurements (on two of the days only, viz., in *Sets* Nos. 3—6, 9, 10) being done *one with each*

Instrument in turn, so that the complete Set of measurements with each Instrument was prolonged over some hours instead of being done (as in all the other cases) in as short a time as possible with any one Instrument.

Notwithstanding these disadvantages, they serve the present purpose quite well enough of showing the variability of the measurements of the same quantity at very various depths (at surface, near middepth, and near the bed) and with very different Instruments.

In the six Sets, Nos. 1, 3, 4, 5, 6, 7, every Float recorded was in "fair course"; but in the last two Sets (Nos. 9, 10) every Float thrown from the Upper Boat was timed—whether in "fair course" or not—and its "Deviation" from the centre line recorded (in order to ascertain the maximum ordinary "Deviation" possible with the two Instruments under trial).

3c. DISCUSSION.—Both Tables show the maximum, mean, and minimum velocity-measurements of each Set, and also the "Range" (or difference between the greatest and least values), and the percentage that this is of the mean. A glance down these last columns (of Range and Percentage) will show at once the *very great variation* of the measured values within the short time required for each Set, viz.,

Central Surface Velocity. The Range is about 25 per cent. in two cases, and exceeds 9 per cent. in all the rest.

Subsurface Velocity. The Range exceeds 12 per cent. throughout, is close to 30 per cent. in 5 cases, and exceeds 57 per cent. in one case.

In seeking an explanation of this enormous Range in the measured values of the same quantity (done in rapid succession in many cases), it seems necessary to inquire whether it is wholly or partly due either to downright MISTAKES* or to the accidental ERRORS* inevitable in all physical measurements.

1°. *Mistakes*. By these are meant such *downright Mistakes* as sometimes occur in timing, in recognizing the Floats at both Ropes when a rapid succession is passing, in entering figures in the Field-book, in reading the Current-Meter dials, in gearing and ungearing the Current-Meter, &c. Such Mistakes may be †*large*, and will therefore suffice to account for large occasional Discrepancies: but they *should be rare with trained Observers*, and will, therefore, in no way sufficiently account for the phenomenon in question, which is the *constant* large variation in successive velocity-measurements.

2°. *Accidental Errors*. These are the *small* ERRORS inevitable in all physical measurements. In the velocity-measurements (done with Floats) in these Experiments, the only material source of small Error (affecting the present question) is the difficulty of timing.

Now it was shown (Ch. IV, 26b), that the Error in timing with the trained Observers on these Experiments might be expected not to exceed one half-second. But the actual difference of the "timings" of the quickest and slowest Floats of a Set was *often* 6, and *seldom less than* 3 *half-seconds*, that is to say, always largely exceeded the greatest ordinarily possible Error in timing.

* In the technical sense of the "Theory of Errors of Observation," (*see* Airy's Work, Art. 5, 8.)

† Thus it seems likely that the unusually large "Range" of 57·3 per cent. in one of the Current-Meter Experiments (No. 8 of Tab LXXIV) was due to an accidental gearing or failure in ungearing the Instrument, an accident to which Moore's Current-Meter was found very liable in a swift stream.

It is considered, therefore, that the precision of the observations is far too great to admit of the great Range in the measured values of the same quantity being (beyond a very small extent) accounted for by ordinary Accidental Observation-errors. The only possible way left to account for by far the greater part of it is *in the variability of the motion* itself.

It was, moreover, quite a common thing for the "timings" of two Floats *even when passing through the 50 foot Run pretty close together* to differ by as much as 3 or 4 half-seconds (a quantity again far exceeding the greatest ordinarily possible error in timing). This shows that the *variation of velocity was very rapid.*

[Some *special* Experiments on this question were fully detailed in the 1874-5 Report, Art. 24. The evidence given above is thought sufficient, so they are not republished here].

3d. VISUAL EVIDENCE.—But there is also independent proof of the great variability of the motion, independent that is of the timing, (and, therefore, independent of errors in timing.)

The practice of executing the Sets of 48 successive central surface velocity-measurements was to throw out three Surface-Floats at a time from the Upper Boat at intervals of about 3′ to 5′. It was quite a common thing for these to arrive at the Upper Rope (100′ distant from the Upper Boat) nearly together, or even in a different order from that of starting. This irregularity may be partly accounted for by the variable effect of the "wake" of the Boat itself on them at first starting: but this effect must have been entirely dissipated before arrival at the Upper Rope. Nevertheless even within the 50′ "Run" itself it was quite a common thing for Floats passing the Upper Rope in the order A, B, C, to pass the Lower Rope nearly together, or even in a different order as B, A, C; B, C, A, or even C, B, A. Occasionally a Float *has been seen* to gain about 5′ on another near it *within the length* of the 50′ Run.

This accounts at once for a difference of 10 per cent. in the velocity of two Floats *even when pretty close together*, and is a useful confirmation of the accuracy of the Results obtained from the timing, viz., both of the *great variability of the motion* and of the *rapidity of the variation.*

Again, few out of a succession of Floats ever moved in lines parallel to the "current-axis". Some edged off to the right, some to the left in no regular order, the lateral "deviation" being sometimes as much as 12′ in 150′, (*see* Sets Nos. 9, 10, Tab. LXXIV ;) some followed a devious course, sometimes edging a little to the right, sometimes a little to the left.

This shows that the velocity is *very variable in direction.*

4. Motion is Unsteady.—The cases quoted are only a very small portion of the evidence available: thus Tab. LXXIII contains only a small selection out of a very large number of precisely similar SETS (reported briefly in Col. 4 of Tab. LVIII to LXX), in the details of all of which (unpublished, and therefore accessible only in the Field-Books) this variability of velocity was *conspicuous.* The same thing was, moreover,

equally conspicuous throughout the whole course of the Experiments: it is not thought necessary to publish details, which would have enormously swelled the Tables. It is considered then that the evidence is simply overwhelming, that this variability or UNSTEADINESS is a *general property* of water in motion in Open Channels; or in other words that—

"The velocity of water in motion is very variable—even at one and the same point—both in direction and magnitude, and the variation is very rapid, *i.e.*, the motion is very Unsteady", ... (4).

"The amount of velocity-variation at one and the same point is liable to be at least ¼th, or 25 per cent., of the mean value", ... (5).

These Results are conceived to be of the highest importance both in forming a rational Theory of Fluid Motion, and also in their practical effect upon the only course possible in Experiment.

4a. PRACTICAL CONSEQUENCES.—It appears then that the velocity at one and the same point is a very variable quantity increasing rapidly, but continuously from a certain minimum through all intermediate values up to a certain maximum, and then decreasing again and so on, and is, therefore, a *periodic* quantity, involving the time t in its functional expression, which should necessarily be—

$$v = f(x, y, z; t), \quad\ldots\ldots\ldots\ldots (6).$$

Thus any single velocity-measurement can only give an "accidental value" of the velocity in question, viz., the particular value at the particular time in question, and if measured a few seconds earlier or later is liable to be *very different*: from which it follows that—

"A single velocity-measurement is of very little practical use", ...(7*a*).
also, "Single velocity-measurements at different points are incomparable", ... (7*b*),

because being "accidental values" they may be values at different phases of their variation, possibly maxima at one point, minima at another point, and so on.

There is indeed one case in which single velocity-measurements at different points are properly comparable, viz., *if measured at same time.*

Example. Suppose the (superficial) Discharge past a vertical or past a horizontal line, or the (cubic) Discharge through a cross-section, were sought. Conceive the lines divided into a great many very short segments of length β, and the cross-section divided into a great many very small parts of area $\mathbf{a}$, and the velocities at middle of each segment β, or at centre of each small area $\mathbf{a}$ measured *all at same instant* in either case. Then the Discharges in question would be *accurately given* as the sums of the partial Discharges $v\beta$, $v\mathbf{a}$, in either case, *i.e.*,

$$D = \Sigma(v\beta), \text{ or } \mathbf{D} = \Sigma(v\,.\,\mathbf{a}),$$

if the number of segments β or of small areas $\mathbf{a}$ were very large.

But as the necessary condition of *synchronous measurement* cannot possibly be secured in actual practice, this case is of no practical importance, and the truth of Results ($7a$, b), is manifest for practical hydraulics.

5. Average Velocity—This new term will now be used with following meaning:—

DEF. The AVERAGE VELOCITY at a point is the average or mean of the velocities at that point taken through a considerable interval of time, or is in fact $\int_0^t vdt \div t$, where t is very large, ... (8).

Accepting then the Result ($7a$), it follows at once that—

"The Average Velocities at different points are the only comparable values of velocity at those points", ... (9),

and—

"The Average Velocity at a point is the only velocity at that point which is of much practical use, and is, therefore, the one to be sought in all velocity-measurement", ... (10).

6. Tediousness of Experiment.—The Conclusion (9) that "Average Velocities" alone are intercomparable, and are therefore always to be sought in all velocity-measurements, must profoundly affect the course of Hydraulic Experiment from the great length of time necessary to determine the Average value at even one point.

The manner in which this Average value is to be sought depends entirely on the Instrument used. Thus with respect to the three Instruments most largely used in modern times—

FLOATS. These give only accidental values. The Average value can only be found by taking the mean of a large number of measurements.

CURRENT-METERS. These give only average values. The Average value is to be found by letting them run for a considerable time.

PITOT'S TUBES. These give only accidental maxima and minima. The Average value is to be found by taking the mean of several maxima and minima.

With whatever Instrument it be done, the process of finding a good Average value for even one point will obviously take a considerable time. One very important practical Result is—

"All Hydraulic Experiments on large bodies of water must necessarily be tedious, and therefore expensive", ... (11).

And this tediousness cannot be got over in any way with Instruments which, like Floats, give only accidental values. The tediousness is least with Current-Meters, which give average values at once by their construction. The most hopeful way of reducing the tediousness and expense of Hydraulic Experiment appears then to be by the introduction of improve-

ment in the construction and mode of handling of Current-Meters, both of which are at present open to many objections, (Ch. XXIII.)

7. LARGE STOCK OF FLOATS.—The Unsteady Motion of the water involves such frequent undue deviation of the Floats from "fair course", that many Floats must be run to secure a small number of "Good Floats". To work with moderate speed, a large stock of Floats must, therefore, be provided to be thrown in succession from the Upper Boat, so as to obviate the necessity of frequently sending the boats to bank to exchange Floats, as the coming to bank is one of the chief sources of delay.

The great bulk and weight, and the consequent cost of this large stock of Floats, (and accessories such as Plank-Trays,) becomes in the case of Subsurface Floats (Double-Floats and Rods) a serious matter. It is, therefore, an important practical necessity to reduce the size, weight, and cost thereof to a minimum.

8. **Change of Conditions,** SETS.—The length of time necessary to deter mine Average Velocities at many points introduces another serious practical difficulty (sufficient to profoundly affect the course of Hydraulic Experiment) in that the "External Conditions" (Depth, Surface-Slope, Wind, &c.) are liable to change within the time in question.

The only practical way of overcoming this difficulty appears to be to make a small number only of velocity-measurements as quickly as possible at each point *in turn*, so that this small number may be secured *under very nearly similar conditions at each one of the group of points* selected. This process may then be repeated, taking all the points in turn, and when done, may be repeated again as often as necessary to make up the Total number of velocity-measurements at each point requisite to obtaining a good Average value at each. In this way each SET of such velocity-measurements will be affected under approximately the same "External Conditions", and the Average of all such SETS will have been effected under approximately the same "Average External Conditions".

[The number of velocity-measurements to be done at each point at one time (before passing on to the next point), was fixed at *three* in these Experiments, (Art. 12)].

9. **Other views.**—This "unsteadiness" of motion has hardly been sufficiently recognized hitherto as a *fundamental* property of water in motion. The unsteadiness is in some cases attributed to accidental causes such as wind, and in others it does not appear (though recognized) to have in any way influenced the Experiments. Thus—

Mississippi Report, (p. 287.) The motion is thus described—

"Besides the great difficulty of taking the observations with sufficient nicety to detect the very slight difference of velocity at the different depths, there is a second cause of failure, namely, an almost constant relative change of velocity at the different depths. The axis* can rarely be at rest; every varying breeze, however gentle, must affect its delicate adjustment, while the stronger pulsations of a high wind must produce an oscillatory movement even greater than that in the tops of the tallest trees. Different floats, therefore, although they may pass *at the same depths below the surface*, may yet pass at *very different distances from the axis*, and thus measure the velocity at very different points of the curve".

Thus in the Mississippi Report the variability of the motion seems to be attributed mainly to the wind.

Bazin Experiments, (p. 23.) The motion is thus treated—

"The motion of water in a canal is much more complex than at first sight appears. If the velocity at a definite point be observed, it is very soon seen to be variable from instant to instant. These variations are sudden: they take place by very quick starts, and are accompanied by slight changes of surface level. * * * Appreciable even by an instrument like the current-meter, which must be in work for several seconds before yielding any indication, they are yet more so with help of Instruments which—like the Darcy-Pitot Tube—show velocity by an active impulse acting only a very short time. It is seen then that they are almost instantaneous, that they constitute true breaches of equilibrium which recur in periods. The velocity at a given point is then only a mere abstraction; it is a sort of mean between very different velocities which follow in rapid succession. The motion is not a continuous phenomenon, and the simple hypothesis of motion in parallel stream-lines, adopted with the view of submitting the facts to analysis is very far from the truth. A stream-line, *i. e.*, a train of molecules moving in succession with equal velocity in the same direction can exist only for a very short time, and is inevitably destroyed by the constant exchange of molecules which take place between its neighbors and itself and by the consequent oblique movements. These irregular movements appear to be produced most in wide sections. * * * * "

But this does not seem to have in any way influenced the course of the Experiments (on velocity-measurement), except in that the oscillations in the Tube were always registered.

Lake River Report (of '69). In the course of these Experiments, clear evidence was obtained as to the existence of *pulsations* in flowing water. Thus—

P. 595, "I have found these pulsations in every stream I have had access to."

P. 599, "The time occupied by a pulsation varies from a few seconds to over a minute, not appearing to be at all regular. They are greater at the bottom than at the surface, &c., * * I found that eddies made no difference with them, &c., * *"

Many instances are also quoted (*ibidem*) of observations (by other Observers), of "pulsations" of velocity and surface-level, some at sea, some in rivers, some in wells, some in small jets. Those of pulsation of velocity are the ones bearing most direct-

* *i. e.*, the axis of the velocity-parabola.

ly on the subject of this Chapter, though the pulsations of level also show the same thing (Unsteady Motion) indirectly.

10. Average from 50 trials.—In the last two columns of the Tables discussed in Art. 3, each SET of velocity-measurements has been divided into two HALF SETS, and the mean of each Half-Set taken out separately. It will be seen that the means of each Half-Set differ from the mean of the whole Set by *comparatively unimportant quantities*, in no case exceeding ·05 (except in Set No. 8 of Tab. LXXIV, which probably contains some mistake, Art. 3c). From this it would appear that—

"The mean of about 50 velocity-measurements done in rapid succession is a fair approximation to the Average Velocity", ...(12).

11. Conclusions applied.—The Course of the present 4 years' Experiments was entirely regulated on these Conclusions, viz., that—

"Only Average Velocities are intercomparable".
"Averages should be formed from about 50 values",
"and should be formed in such a way as to eliminate personal equation".

The velocity-measurements were accordingly done in certain groups, which will for shortness be termed SETS, as explained in following Articles.

12. Velocity-Work, SETS.—The ordinary *systematic* velocity-work consisted (as explained Ch. I, 12) of—

"Serial Velocity-Measurements upon a vertical or transverse line".

These were executed in following order, (based on the principle laid down in Art. 8).

The velocity-measurements were made in groups of three *at each point in turn, in as rapid succession as possible* from end to end of the line in question, thus—

On a vertical.	*On a transversal.*
3 at surface.	3 at point nearest left bank.
3 at 1′ depth.	3 at next point.
3 at 2′ depth,	3 at next point,
and so on; lastly	and so on; lastly
3 at point nearest to bed.	3 at point nearest right bank.
[Also 6 Rod-velocities, as hereafter explained].	

The complete group of 3 velocity-measurements at each point in turn from end to end of any Line (whether vertical or transversal) will be styled a SET of velocity-measurements on that Line. The Observers (Caller and Timekeeper) retained their places throughout the Field-work of any one such Set.

[Practical convenience required occasional slight modifications from the precise order stated above, not affecting the principle laid down. These will be explained in their proper places].

The only other *systematic* velocity-work was central surface velocity-measurements. These were done in groups of 48 in as rapid succession as possible, each Observer

acting as Timekeeper in turn for half the group. Each such group will be styled a SET of central surface velocity-measurements. Each such SET is obviously freed from the effects of personal equation by the interchange of Observers.

12a. FIELD-WORK OF ONE SET.—The complete Field-work of one SET of velocity-measurements involves noting the state of the Water-level and Wind (Ch. V, 2—10 & 21) at beginning and end of the velocity-work. This was always done in following order :—

FIELD-WORK OF ONE SET.
- 1°. Note of direction and velocity of wind.
- 2°. Note of water-level, (say a gauge-reading.)
- 3°. Complete SET of Velocity-measurements, (as above.)
- 4°. Note of water-level, (say a gauge-reading.)
- 5°. Note of direction and velocity of wind.

The Mean of Nos. 2° and 4° is what is *tabulated* in all the Tables as the (Mean) Gauge-Reading (Ch. V, 10, 12) of the SET. Both states (1° and 5°) of the Wind are entered in the Detailed Tables.

The complete SET was always done as rapidly as possible, and may, therefore, be considered—

"A piece of Field-work done under nearly constant External Conditions".

12b. SEQUENCE OF SETS.—As soon as one SET—of serial velocity-measurements upon the same line (vertical or transverse)—was finished, a second SET was undertaken, after that a third, then a fourth, and so on, *so long as the water-level remained nearly constant*, and the *wind moderate ;* this repetition of similar work (under nearly similar External Conditions) was carried on throughout the working hours of the same day, and again on the next day, and on other days (whenever the water-level suited) up to a limit of about 16 SETS. The Observers (Caller and Timekeeper) always interchanged for successive SETS.

[If the water-level changed more than $\frac{1}{10}$ of a foot, or the wind exceeded 15′ per second, Field-work was usually closed].

The Surface-Slope was also taken usually once a day as explained in Ch. VII, 8.

12c. TABULATION OF SETS.—The Means of the three velocity-measurements at the several points on any one Line may be looked on as—

"Rough Velocity-measurements at each point of the line".

These Means or "Rough Velocities" for every point in any one Field-work SET are entered in Col. 6 of the Detailed Tables, *all in the same horizontal line*, one Sub-column being appropriated to each point.

The Date, state of the Wind at beginning and end of the SET, and Mean Water-Level (of beginning and end of the Set), and certain other Data (to be explained hereafter) peculiar to the Set, are all entered on that same horizontal line.

Thus *each horizontal line* of any of the Detailed Tables contains all the *Data and Results of one Set.*

13. Series.—All such SETS of serial velocity-measurements on the same (vertical or transverse) line as were executed in sequence (Art. 12b) under somewhat similar External Conditions, (especially as regards water-level,) were combined into one SERIES by being tabulated on the same Sheet, (*see* any of the Tables VII to LXX).

[A Range of about ·3 foot of water-level is the maximum that has been admitted].

Each Velocity Sub-column (Col. 6) of these Tables contains, therefore, the Rough Velocities at one point (as marked at the head of the sub-column) of the line in question, viz.—

i. *Velocities past a vertical.* At a definite depth (z) on that vertical,

ii. *Velocities past a transversal.* At a definite distance ($\pm y$) from the centre, and the Means of these Sub-columns (line v of the Tables) are the best measures of the AVERAGE VELOCITIES at the several points which the ever-varying state of the Canal and of the Wind permitted to be obtained.

[Thus in many of the SERIES, the Average Velocities at the foot of the Table are the Average of 16 SETS, *i.e.*, of ($16 \times 3 =$) 48 velocity-measurements. The limit of 16 Sets was in fact chosen so as to make the Means depend on about 50 distinct trials, as proposed in Art. 10. In very many cases on the other hand, (especially at low water,) the ever-varying state of the Canal permitted of the accumulation of only a few Sets—sometimes only one—under nearly similar External Conditions].

Hence though individual SETS may have been done under somewhat different "External Conditions", the Average of each SERIES is itself really a SET of AVERAGE VELOCITIES obtained *under the same Average External Conditions,* and nearly freed from personal equation of the Observers, (in consequence of the frequent interchange of Observers, Art. 12b.)

13a. ORDER OF SETS.—As the SERIES were originally formed during the progress of the Experiments, the SETS intended to be combined were of course entered upon the Tabulation Sheet of the SERIES in order of date, *i.e.*, in the order of execution : but they have been re-arranged for publication partly by order of depth, so as to exhibit better the effect of change of depth upon the velocities. The work of each single day is entered commonly by order of execution, but the work of different days is arranged usually by order of depth : slight variations (such as ·01 of a foot) of water-level being, however, disregarded.

14. Set-Combination.—It will be seen that the *only combination used* has been that of SETS of similar work, under tolerably similar conditions, as follows :—

1°, at the same* Site.

2°, upon the same line (*i. e.*, upon the same vertical, or same transversal).

3°, at nearly the same water-level, (allowing a range of $\frac{1}{10}$ of a foot).

4°, with surface-slopes not very dissimilar, or with nearly equal mean velocities.

[It would have been certainly desirable to combine only such SETS upon the same vertical or transversal as were effected under closely the same "External Conditions", *i.e.*, at nearly the same water-level, same surface-slope, and same state of wind. But the ever-varying states of the Canal and of the Wind rendered compliance with these

* *Exception.* In one case an exception has been made to this Rule (of not combining Results from different Sites), viz., Surface Velocity Ser. No. 60 at the Solâni Embankment Minor Sites, which is made up of 5 Sets at the Upper, and 5 Sets at the Lower, Site. This combination was thought justifiable on account of the similarity of Average Cross-Sections of these two Sites, (Ch. III, 11).

conditions practically unattainable, as far as the state of Wind and Surface-slope are concerned, and only approximately as regards the Water-Level. In fact, in order to obtain a large enough number of SETS (about 16)—at nearly the same water-level—to yield fair measures of AVERAGE Velocity, it was found necessary to combine the SETS into SERIES, *irrespective of the state of the wind*, and to some extent *irrespective of the surface-slope;* the effect of change of surface-slope has been partially allowed for by not combining any SETS in which the mean velocities differ by a large amount, an effect which would certainly be caused by any considerable change of slope].

It is submitted that the Combinations used are far more fairly comparable than the Combinations that have been used in any Experiments on large bodies of water hitherto published.

[The fact is that in Experiments on large bodies of water, the uncertainty of the frequent recurrence of closely similar "External Conditions" is so great, that the time available for systematic Experiment has never sufficed to accumulate any considerable quantity of Experiments under closely similar External Conditions, and it has in consequence been unfortunately necessary to combine Experiments even under very dissimilar External Conditions, in order to obtain some sort of Average Velocities].

14a. *Combinations in other Experiments.*—The modes of combination actually used in the modern Experiments on large Rivers are briefly explained below.

14b. *Vertical Curves.*—In the Mississippi, Connecticut, and Irrawaddi Experiments, the practice was to combine SETS *on different verticals*, and *in all depths of water, irrespective* also *of state of surface-slope* and *of wind.* The combination was effected in some one of following ways :—

1°, between actual velocities at *actual* depths,

2°, between actual velocities at *proportionate* depths (*e.g.*, at each tenth of depth,)

3°, between *relative* velocities at *proportionate* depths ;

the term "relative velocity" being taken to mean the ratio of an actual velocity to some one principal velocity (*e.g.*, to the mean velocity) taken as unit.

Modes 1° & 2° were used in the Mississippi Report (pp. 230, 232), and in the Irrawaddi Report (for '75, pp. 18, 19, & Tables, *passim*,) and Modes 2° & 3° in the Connecticut Report (for '78, p. 313).

[In the Mississippi Experiments the combination was frequently of Sets done *even at different Sites, see* Report, pp. 230, 232].

The legitimacy of these combinations depends of course on the (unproved) assumptions that the figure and size of the Average Velocity-Curve is the same *upon all verticals*, (even in different Sites,) and *at all depths on those verticals*, and under all changes of surface-slope and wind. That this is *not generally true* is abundantly shown in the Darcy-Bazin and in the present Experiments. A mere glance at the Diagrams of Average Vertical Velocity-Curves (Pl. XII to XVIII) will show that these Curves differ not merely in size, but also in figure for different verticals, and even for the same vertical with different depths of water thereon.

14c. *Transverse Curves.*—In the Mississippi Report (p. 236) the only Transverse Curve studied appears to have been that whose Base-Transversal was at 5′ depth, comprising, therefore, velocity-measurements at 5′ depth from bank to bank.

The practice appears (p. 236) to have been to combine all SETS whose mean velocities lay between certain limits (between 1′ & 2′, 2′ & 3′, &c., *per sec.*) *in all states of water-level*, and *irrespective of the state of wind.* The Average Velocities were formed as the Means of the Actual Velocities in Float-paths anywhere within the same 200′ division of the Base-Transversal (pp. 236, 226, 227).

[No combinations of Transverse Curves appear to have been used in the Connecticut, Lake River, or Irrawaddi Reports : and none of either kind (vertical or transverse) appear in the Révy Report].

15. Average Velocity-Curves.—The velocity-measurements of any one SET (or of any one line of a SERIES) being only Rough Averages (means of only 3 measurements) give—as might be expected—when plotted, very irregular Curves, which may be termed ROUGH CURVES, of no use for tracing geometrical properties. And if several such Curves of same kind be plotted, they will be *found to differ considerably in detail*, (even though under nearly similar External Conditions,) though agreeing in certain general features, viz., in such a way that if plotted on the same Base-Line, they appear to be inextricably* interlaced. This will be recognized to be a consequence of the Unsteady Motion, whereby only AVERAGE VELOCITIES are fairly comparable.

The means of the Velocity Sub-columns in each SERIES being the best measures of the AVERAGE VELOCITIES practically attainable, the figure which results from plotting them may be termed the AVERAGE VELOCITY-CURVE, each Curve being particularized according to its Base-Line, *i.e.*, either as—

Vertical, Surface, Middepth, Bed, or Mean (Velocity-Curve).

A glance at the delineations of these Curves—

Vertical, Pl. XII to XVIII ; Surface, Pl. XXVI to XXVIII ;
Middepth, and Bed, Pl. XXIX ; Mean, XXXI to XLI,

will show that (when derived from a sufficient number of SETS) they certainly approximate to being tolerably regular curves, *i.e.*, curves with tolerably regular changes of curvature, also that the *regularity of figure increases generally as the number of Sets in a Series increase.*

[The number of SETS used for each Curve is indicated in the Diagrams].

This leads to the Conclusion that it is a general law of flowing water *in a long uniform Reach with a Bed of regular contour*, that—

* This was fully shown in Art 30, 58, Pl. III and VII of the 1874-75 Report, *q. v.*: it has not been thought worth while to repeat the discussion here.

"The Average Velocity-Curves are pretty regular curves, and are generally everywhere convex down-stream, (except where modified by certain causes discussed hereafter,)" .... (13),

and that the departures from this regularity in the actual drawings are due partly—

1°, to the insufficiency of the number of velocity-measurements for yielding fair values of the Average-Velocities.

2°, to the irregularity of contour of the bed and banks at the Site, and also to irregularity of the channel above and below the Site.

As to 1°, want of time and opportunity prevented in many cases—especially at low water—securing the full number 16 SETS fixed on as desirable for a SERIES. Thus some of the SERIES consist of *only one or two* Sets,

Vertical Curves,* Ser. 10, Pl. XIII.

Surface Curves, Ser. 55, 59, Pl. XXVI, XXVII;

Mid-depth Curves, Ser. 62, Pl. XXIX.

Mean Velocity Curves, Ser. 113, 115, 124 to 127, 131 to 139, 158, 164, 168, 181, 191, 194, 195, 197, Pl. XXXII—XXXVII.

Most of these Curves are—as might be expected—very irregular. They are in fact drawn only to exhibit prominently the irregularity of figure inherent in the use of a very limited number of measurements (3 to 6) for each velocity-ordinate.

Again, as to 2°, the irregularities of curvature due to these causes are very marked in the case of the Transverse Velocity-Curves; these will be fully discussed in Ch. XVII].

It appears then that for the purposes of any useful discussion of the figure of the Velocity-Curves—

"Curves derived from numerous SETS of data are entitled to more weight than those depending on only a few SETS",.. (14).

16. Discharge-measurement, FAIR AVERAGE.—The various Discharges computed in the sequel, viz.,

1°, Discharge past a vertical; 2°, Discharge past a Transversal;
3°, Cubic Discharge,

have been computed separately for each line of the Detailed Tables, *i. e.*, for each SET of Field-work, and, therefore, depend only upon what have been styled Rough Velocity-measurements. Such single Discharge-measurements, being Results of single Sets, cannot be expected to be good Averages, but must be looked on as FAIR AVERAGE values.

These FAIR AVERAGES are, however, much nearer approximations to the true Average values than is likely to be the case with the primary velocity-measurements on which they depend, inasmuch as—these being taken at random at all phases of their variation—a sort of compensation ensues in the Discharge computed from them all.

* Only one such case has been plotted for the Vertical Curves. Instances might of course have been multiplied, but it was not thought worth while.

[In the case of Discharges past a Transversal and Cubic Discharges, the velocity-measurements were effected (in the wide channels) in from 15 to 19 distinct Float-Courses, so that these Discharges depend on about (from 3 × 15 to 3 × 19) 50 velocity-measurements ; so that such Discharge-measurements, and their corresponding Mean Velocities, are probably fair approximations to Average values].

Similar remarks apply to all the Mean Velocities computed from these Discharges in this work, whether—

1°, Mean Velocity past a Vertical; or 2°, past a Transversal;
or 3°, Mean (Sectional) Velocity.

A glance at the lines (marked δ) of "Ranges" of the velocities throughout the Detailed Tables will show in fact at once that, as a general Rule,

"The Range of the Mean Velocity-measurement < Range of the detailed Velocity-Measurements", ... (15).
[Further evidence will appear in Chap. XXI on Discharge-Verification].

17. Stream-lines interlace, (Art. 3d, also Ch. IV, 21).—The great irregularity of the paths of successive Floats is a proof positive that the stream-lines interlace freely in a horizontal plane.

Experiments with Floats are not suited to showing the fact of vertical interlacing. But this is readily observed in a slightly silt-laden stream wherever eddies exist: clouds of silt can be seen boiling up from the bottom, rolling over, and plunging wildly in all directions. It might be supposed that this was a condition attending the existence of eddies. It has, however, been shown* by direct Experiment in straight reaches of two pretty uniform canals, that there is a *continual transfer of water from the bed towards the surface* even in water *in apparently tranquil motion*; this of course involves a continual transfer from the surface towards the bed.

[The Experiments consisted in discharging a few cubic feet of whitewash into the canal near the level of the bed from a pipe leading out of a reservoir in a boat moored at mid-channel, the lower orifice of the pipe being about 5" above the bed].

It is considered then to be proved that—

"The stream-lines of water apparently in tranquil motion interlace freely in all directions", ...(16),

from which it follows that the ordinary hypothesis, on which most of the common hydraulic formulæ are based, of a state of flow in parallel straight lines is a condition *having no physical existence* in large Open Channels.

* See "Trans. of Amern. Socy. of Civ. Engrs.", Vol. VII of '78, No. CLX, "On the Cause of the Maximum Velocity of Water flowing in Open Channels being below the Surface", by J. B. Francis.

18. Average Steady Motion.—Notwithstanding the great variability of the motion *in detail*, the "Average Velocity" at a point is probably a constant quantity, that is to say—

"There is Average Steady Motion",..(17).

The evidence on this point is indirect. The pretty close agreement of the means of HALF SETS of velocity-measurements (*see* Art. 10) done in immediate succession shows that—

"The Average Velocity at a point is probably constant (under similar External Conditions)",..(17*a*).

The irregularity noticed (Art. 15) of Velocity-Curves plotted from single velocity-measurements, and the greater regularity of those plotted from repeated measurements—a regularity increasing with the number of repetitions—point to the same Conclusion.

Again, irregular as is the path of each single Float, and although Floats started in rapid succession move in very different paths, still the "average path" in a long straight reach seems to be pretty constant, *i.e.*, parallel to the so-called current-axis.

All these facts together tend to prove the truth of the above Result (17), and it would seem, therefore, that AVERAGE VELOCITIES are inter-comparable *as if there were Steady Motion*.

[This comes out also very clearly in Experiment ii of Ch. IV, 28b, on a series of 48 Surface-Floats timed through two 25', a 50' and a 100' Run : although individual Floats differ a good deal in the different Runs, the means of the 48 Floats agree extremely closely over all four Runs].

19. Wind-motion, Analogy.—Any one watching a common wind-vane cannot fail to remark how incessantly it sways about—sometimes suddenly shifting through 90° and back—thus showing great variability of direction.

A similar variation in intensity may be noticed either by watching the incessant rising and falling of a pennon fluttering in the wind; or again by simply listening to the wind, the sound of which is notoriously ever-varying. The same thing will be noticed in watching an anemometer: the vanes will be seen sometimes to spin rapidly round for a brief space, then slowly, anon fly round awhile.

Thus the motion of wind is—by common observation—ever very variable, or is technically very UNSTEADY. And yet there is a certain average direction, and average velocity—if observed for some seconds—which are commonly termed *the* Direction, and *the* Velocity of the wind at the time.

The analogy with the motion of water in a confined channel is of course far from perfect: still the characteristic of UNSTEADY MOTION is common to both. In the case of wind it is familiar, while in that of water it is only not so obvious.

20. EFFECT ON THEORY.—The Conclusion (4) that Unsteady Motion is a *fundamental property* of water in motion in Open Channels is of great importance in attempting to form a rational Theory of fluid motion, and enormously increases the difficulty of it. No Theory in which this is not included as a fundamental property can have much hope of success.

The attempt has been made in M. Boussinesq's large Work "Essai sur la Théorie des Eaux Courantes", by making allowance for the effects of the Unsteadiness in forming the "Equations of Motion", as below explained, (pp. 6, 7, *op. cit.*)

"It is necessary then,—if we wish that Hydraulics should no longer be (as M. de Saint Venant puts it) a *hopeless enigma*,—1°, to regard the real velocities at points within a fluid in motion as rapidly or even abruptly changing from point to point; capable in fact, of producing frictions of an order of magnitude quite different to the case of continuous motion; 2°, to make the mean actions exercised across a fixed plane element depend not only on the *local average velocities*, or rather on their first differentials which measure the mean relative slidings of the fluid strata, but also at each point on the intensity of the whirling action there obtaining; 3°, to seek, therefore, the causes on which the whirling action might depend at different points of a section, and to make the co-efficient of internal friction vary with them; to choose finally for equations of motion, not the relations which express at a given moment the dynamic equilibrium of the different fluid volume-elements, but the means of these relations during a pretty short interval, or what might be called the Equations of mean dynamic equilibrium of the fluid particles which successively pass any one point".

It is on some such bases as these, viz., use of AVERAGE VELOCITIES, and of AVERAGE STEADY MOTION, that a rational Theory must be founded. Unfortunately the ordinary formulæ of Hydraulics are based on the hypothesis of a Steady Motion which has in fact no existence.

Addendum to Art. 14b.

In the Lake River Experiments SETS *on different verticals* and *in all depths of water* were combined by Rule No. 1°, (*see* Lake River Report of 1870, pp. 570 & 574—592).

CHAPTER VII.

SURFACE-SLOPE.

Preface.—This Chapter contains a detailed description of the mode of measurement of Surface-Slope (Art. 2—4a), of Surface-Fall in long stretches (Art. 9, 9a), and of state of Control (Art. 10—13g), with a detailed discussion of the Results (Art. 5—7a, 14—17). The Articles containing the Conclusions (Art. 2a, 2b, 5a, 6a, 7a, 10c, 11a, 15—15c, 16e, 17) are the most interesting.

1. Bed-Slope.—The slope of the bed of an open channel is perhaps the most important of the *primary circumstances* affecting the flow of water through the channel.

Uniform Bed-Slope.—In a channel with uniform bed-slope, and bed not liable to erosion, the term Bed-Slope has an obvious definite meaning.

Irregular Bed.—But in a channel with an irregular bed, or a bed which is liable to erosion and to silting, the best meaning to ascribe to the term is by no means so obvious. The accidental irregularities of the bed are frequently so great, that *in any short distance* there may be no fall but actually a rise, (*see* Pl. I, & IV to VI.) In such a case, the Local Bed-Slope would seem to be a quantity of little practical interest; and the AVERAGE BED-SLOPE (through a long distance) must be considered instead. In a channel like the Ganges Canal, the bed of which is *permanently defined* at short intervals by the masonry floorings of frequent Bridges and Falls, the best meaning to ascribe to this term would seem to be as follows :—

$$\text{Average Bed-Slope} = \frac{\text{Fall of Bed between two adjacent permanent floorings}}{\text{Distance between those floorings}}, \quad (1).$$

This is of course a constant definite quantity for the Reach between any two adjacent floorings.

Obstructed Falls.—The presence of "Obstructed Falls", *i.e.*, FALLS with crests raised above the general Bed—the common type on the Ganges Canal—introduces, however, a special difficulty as to how to apply the term *in the neighborhood of* such Falls.

2. Surface-Slope.—The SURFACE-SLOPE in an open channel is perhaps the most important of the EXTERNAL CONDITIONS (Ch. I, 13) which determine the velocity.

It is convenient to employ the following terms :—

$$\text{AVERAGE SURFACE-SLOPE} = \frac{\text{Fall of surface between any two points}}{\text{Distance between those points}}, \ldots (2).$$

LOCAL SURFACE-SLOPE = Average Surface-Slope *in a very short distance*, (3)

The latter quantity—the Local Surface-Slope—is the most important for THEORY, and is the one required (in strictness) *in all formulæ* involving surface-slope. But the former, the AVERAGE SURFACE-SLOPE, is the only one admitting of direct experimental determination.

SURFACE-GRADIENT.—This term will be used to denote either Fall or Slope of water-surface in a general sense where precision is not required.

2a. Slope-Points, Slope-Length.—From the definitions it is seen that Surface-Slope measurement depends on the accurate determination of the Surface-Fall between two points in a line parallel, if possible, to the Current-axis. It will be convenient for shortness to denote the two points in question by the name SLOPE-POINTS, and the distance between them by the name SLOPE-LENGTH.

Further, it is clear that the Surface-Fall should be so measured that the Result, which is of course an Average Surface-Slope, should approximate as closely as possible to the Local Surface-Slope. This involves that—

"The Slope-Length should be the shortest compatible with accuracy in estimating the Surface-Fall", ...(4).

The Field-work of the Surface-Fall measurement usually resolves itself into finding the difference of level between each of the two SLOPE-POINTS and two masonry bench-marks on the banks at a short distance from them: the difference of level of these latter having been accurately determined once for all. Thus the sole *regular* Field-operation is ultimately—

"The accurate determination of the Water-Level at the two Slope-Points",

the manner of doing which has been fully described in Ch. V, 3—9.

In actual practice, the two SLOPE-POINTS are always necessarily two points in the water-surface *close to the banks*, the practical determination of the water-level at points distant from the banks being one of great difficulty, (*see* Ch. VIII, 3b.)

2b. *Delicacy of the work.*—The Surface-slope of all large bodies of water in motion in earthen channels in a plain country is for the most part (*i.e.*, except near rapids, falls, &c.) such a minute quantity, as to require the greatest delicacy of observation to determine it at all accurately. The oscillations of the free water-level—noticed in Ch. V, 5—are a serious

difficulty in the way, necessitating the use of a far greater Slope-length than would be otherwise necessary, so that—

"The Slope-Length should be such as to yield a Surface-Fall greatly exceeding the ordinary oscillation of the free level", ... (5).

This condition is of course inconsistent with (4) above, and a compromise must be made.

[Thus in these Experiments there were associated with each Experimental Site, a pair of SLOPE-POINTS on each bank, one at 1000′ above, and one at* 1000′ below the centre section of the Site, giving, therefore, a standard* SLOPE-LENGTH of 2000′ on each bank].

To meet the difficulty of the *rapid* oscillations of the Surface at all Slope-points in the free channel, and which, though subdued, are not wholly lost in the so-called "still water pools", and also to meet the difficulty of possible gentle oscillations of long period, (which, therefore, commonly escape notice,) it seems further essential that—

"The water-levels at the two Slope-points should be determined *simultaneously* by two Observers", ...(6).

In order also that the resulting Surface-Slope may tolerably represent the Surface-Slope *at the centre cross-section* of the Experimental Site, it seems further essential that—

"The Slope-points should be *equidistant* from the centre section of the Experimental Site, and the channel should be *symmetrical*—both geometrically and physically—about that section throughout the Slope-length, and for some distance above and below the Slope-points", ...(7).

And it is of course obvious that—

"The two Slope-points should (unless still water pools can be arranged) be situate at such parts of the banks as are nearly free from eddies and backwaters, and where the motion of the water is as quiet and equable as possible", ... (8).

Inasmuch as water-level is also slightly affected by velocity (*see* Ch. V, 2, 8), it would seem further necessary that—

"The Slope-points should be so situate that the surface-velocities past them are nearly equal", ... (8*a*).

[Strict compliance with this last Condition is not attainable with any certainty in practical work ; it can only be approximated to somewhat roughly].

[The important conditions Nos. (7), (8) were *fulfilled to a high degree* in the case of the principal Experimental Sites, viz., the Solání Embankment and both Solání Aqueduct Sites, in consequence of the great uniformity of the banks throughout the whole length, 2¾ miles, of the Solání Embankment (for Cross-Section, *see* Pl. II) ; and to as great an extent as is possible in earthen channels, (the banks of which are always somewhat irregular,) *at all the other important Sites* except the Belra Site : the Belra

* By an oversight the lower length was set out 1060′ at the Kamhera Site, thus giving a Slope-length of 2060′ thereat. This does not sensibly affect any of the Results.

Bridge unfortunately fell within the upper Half Slope-length (at this Site), and may possibly have made the surface-slopes observed at Belra somewhat abnormal].

2c. *Other Experiments.*—As to the exceedingly delicate nature of such work, the necessity of simultaneous observation at the two Slope-Points, and the precautions taken by others, *see*

1°, *Miss. Report*, pp. 302, 303, 314; also *Abbot's Gauging of Rivers*, pp. 312, 313.
2°, *Lake River Report* of '70, pp. 568, 570; *Connecticut Report* of '78, p. 350.
3°, *Révy Expts.*, pp. 72, 122, 144; *Irrawaddi Report* of '79, p. 135, *et seq.*

In the Mississippi and Lake River Reports, the observations at the two Slope-Points are distinctly stated to have been *simultaneous*. The absence of such statement in the other Reports makes their published Surface-Slope measurements less valuable.

2d. FIELD-WORK.—The actual Field-practice for Surface-Slope measurement was as follows:—

The two Observers started at the same time from the Experimental Site so as to arrive at the Slope-points at opposite ends of the Slope-length nearly at the same time. On arrival at the Slope-points (2000′ apart) they *signalled to each other*, and then both knelt down at once on the bank near the water's edge, and took the water-level at the two Slope-points as explained in Ch. V, 7, by applying the thin brass Foot-Rules upon the (permanent or temporary) Bench-mark just below the water-surface, marking with a pencil on the face of the Rule the "highest maximum" and "lowest minimum" water-level which occurred *within the same short space of time*, say about half a minute, (so that the two water-level determinations were as nearly as possible simultaneous,) and also the distinctive number of the Bench-mark used.

On return to the Experimental Site, the marked numbers were always at once entered in the Field-Book: and to prevent risk of *mistakes* (*in reading* the graduations of the Rules), both Observers examined each Rule, and were held *jointly responsible* for the correctness of reading the graduations.

[The only downright "mistake" likely to elude subsequent detection seems to be the accidental misplacing of the Foot-Rule upside down on the permanent or temporary Bench-mark, which would involve reading ·6 for ·4, ·7 for ·3, and so on. This mistake may possibly have occurred *occasionally* at the Sites in the Roorkee Reach, (and may *perhaps* be the cause of certain anomalous Results at these Sites): but it cannot possibly have occurred in any of the work done at the other Sites, because the Rules were purposely *cut* (at the top) before any Field-work was done out of the Roorkee Reach, so as to remove any chance of this mistake.

A "mistake" in marking on the Rule at the time of use the proper number of the (permanent or temporary) Bench-mark used would have led to a change of the deduced Surface-Fall of at least 9″, which would be at once recognized as an incorrect result on working out in office].

2e. SURFACE-SLOPE ENTRIES.—The Surface-Slope obtained as above is obviously—

$$S = (\text{Surface-Fall in } 2000') \div 2000', \qquad (9).$$

The Results are always very small, never exceeding ·000480 in this Work, and, therefore, always containing at least 3 (and sometimes 4) ciphers before the first sig-

nificant figure. To save space in printing, *only the last three figures* are entered in the Sub-column of "Local Slope" (S), (*e.g.*, Col. 8 of Tab. VII to LV, and Col. 5 of Tab. LVIII to LXIX, &c.),

These entries can, therefore, obviously be read in two ways: thus take the entry 815 in the Sub-column of S: this may be read—

Surface-Fall in 1000′ = ·315 *feet* (the decimal point being prefixed by the reader).

Average Surface-Slope S = ·000315, (3 decimals being prefixed by the reader.)

As to the mode of *repeating* entries by commas („), *see* Vol. II, Art. 6 of Introduction.

3. Water-level.—The water-level was determined at all the SLOPE-POINTS used in these Experiments by the "Temporary Free Gauge" Methods described in Ch. V, 7, *q.v.*; the arrangement was in all the cases such that the Average WATER-LEVEL as defined in Ch. V, 5, (viz., the mean of the "highest maximum" and "lowest minimum" within a short space of time (say about half a minute) admitted of *very accurate determination*, (except in a high wind.)

The actual mode of taking the water-level at the Slope-Points of each Experimental Site is shown below.

SITE.		Slope-Points. Banks.	Slope-Points. Position.	Mode of taking water-level.	Bank most used.
15th Mile	New Site,	Both	In free channel	As for earthen channels, Ch. V, 7c,	Both
15th Mile	Old Site,	Left	„ „	„ „ „ „	Left
Solání Sites	Embankment, (Main Site)	Both	„ „	As for Solání Embankt., Ch. V, 7b,	Left
Solání Sites	Left Aqueduct, ..	Left	„ „	„ „ „ „	Left
Solání Sites	Right Aqueduct,	Right	„ „	„ „ „ „	Right
Belra, ...		Both	In still pool	As for earthen channels, Ch. V, 7c,	Both
Jaolí, ...		Left	In free channel	As for Solání Embankt., Ch. V, 7b,	Both
Jaolí, ...		Right	„ „	As for earthen channels, Ch. V, 7c,	Both
Kamhera, ...		Both	In still pool	„ „ „ „	Both
Solání Embankment, (Minor Sites)		No slope-measurements taken.			
Distributaries, ..		No slope-measurements taken.			

4. Levelling.—From the smallness of the quantity (Surface-Slope) to be found, the utmost possible accuracy is required in the levelling operations for determining the differences of level between the permanent masonry Bench-Marks at the 4 Slope-points, and also between these and the series of "Temporary Bench-Marks" used (Ch. V, 7) for determining the water-level.

The whole of the *more important* levelling over long distances, viz.—

1°, connecting Bench-marks at different Experimental Sites not very far apart, as in the Roorkee Reach;

2°, connecting the masonry Bench-marks at the Slope-points 2000′ apart with each other and with the Experimental Site Gauges;

was done with an excellent 20″ Troughton Level *with the utmost care*, at least twice over, once by one Observer, and once by a second Observer. In the case of the 4 Slope-points associated with each Experimental Site, a *complete circuit* of levels (embracing the 4 points) was also invariably made by each Observer; so that each Observer's levelling checked itself. In the case of discrepancies exceeding ·01 of a foot in the circuit, the levelling was repeated.

4a. *Connection of opposite banks.*—These were invariably connected by the following process which eliminates all errors of adjustment of the Instrument, and also the effects of curvature and refraction at same time.

Supposing A, B (Pl. XX, 6) two Bench-Marks, the difference of level between which is to be accurately determined, and that being on opposite banks of a canal (or from any other cause), it is impossible to place a Level *between them*. Two equal distances Aa, Bb are to be laid out from A, B respectively in such directions that the triangle AaB, BbA on same base AB may be similar and equal to each other, but oppositely situate. The same Level is to be set up at both points a, b in turn, and readings taken *from both* upon a single Levelling Staff set up (on both occasions) at both points A, B: the Level-Stand should be approximately similarly placed with respect to the base line AB on both occasions.

The difference of level of the Bench-marks will thus have been twice found; once with the Level at a, and once with the Level at b. The two Results will be equal, and will be the true difference if the Instrument be in perfect adjustment, but not otherwise: but the arithmetic mean of the Results will be the true difference of level (independently of all errors* of adjustment of the Instrument, and freed from effects of curvature and refraction).

[This will be sufficiently obvious from the reflection that—

1°, each point A, B has been observed once with the same long focus, once with the same short focus.

2°, the angular movement of the Instrument is the same at both points a, b].

5. Slope-Length, EXPERIMENTS.—A few (12) Experiments were made (at the Solání Sites) in 1876 on the effects of using different Slope-Lengths, by taking water-levels at four Slope-points on the same bank, viz.—

At 2000′ and 1000′ above, and also at 1000′ and 2000′ below an Experl. Site, thus giving two Slope-Lengths of 2000′ and 4000′ respectively, *symmetrically situate about the centre* of the Site. These fulfilled in a high degree the conditions (7), (8) favorable for Slope-measurement. The Details are given in Tab. LXXIX.

The Table shows the Surface-Fall in each 1000′ length separately, and the resulting

* This was verified by the author with an Instrument purposely thrown excessively out of adjustment.

Surface-Slopes (3 decimals ·000 to be supplied by the reader) in the 2000′ and 4000′ Slope-Lengths.

The Experiments (11 in number) at the Solání Right Aqueduct Site are valuable, as covering a wide Range of the Gauge (10′·0 to 4′·3), and done in calm or nearly calm air.

[For perfect comparability, the water-levels should—to meet the difficulty of the oscillations—have been taken simultaneously at all four Slope-Points. This was, however, impossible with the available Staff of only two Observers ; so that a compromise was made by taking the *water-levels concerned in each individual Slope-measurement, i.e.*, at the two ends of either Slope-Length *strictly simultaneously*, so as to make each Surface-Slope measurement good in itself (*see* (6) above). Thus the water-levels at the Slope-Points of the 4000′ Slope-Length were taken about 5 minutes later than those at the Slope-Points of the 2000′ Slope-Length, (this being about the time occupied by the Observers to walk from the latter Slope-Points to the former.) The Surface-Slopes in the 4000′ Slope-Lengths, may, therefore, be said to have been measured about 5 minutes later than those in the 2000′ Slope-Lengths].

5a. *Conclusions.*—It will be seen (*see* Table) that the Surface-Falls in the four 1000′ spaces differ by quantities greatly exceeding the probable errors of the work, and that the Surface-Slopes deduced from the 2000′ and 4000′ Slope-Lengths also differ in consequence considerably, and pretty steadily in the same direction. This shows that—

"Non-simultaneous Average Surface-Slope measurements deduced from different Slope-Lengths (even when symmetrically situate about a common central line) are liable to differ considerably, (even when not differing by more than 5 minutes' interval), ... (10),

and confirms the preceding Results (4), (5) that to obtain Local Surface-Slope, the Slope-Lengths must be the shortest compatible with accuracy in estimating the Surface-Fall. Result (10), however, *seems to preclude the hope of obtaining true Local Surface-Slope measurements* with any certainty, so that the following procedure seems absolutely essential—

"To render Surface-Slope measurements in any way fairly comparable, the same Slope-Length should always be used at any one Site", ...(11),

and it would probably be better to use the same Slope-Length for all Sites.

6. Surface-Slopes at both Banks.—In a few cases in the Roorkee Reach, and on nearly every occasion of Discharge-measurement in the Belra, Jaolí, and Kamhera Reaches, Surface-Slope measurements were made *on both banks*, as follows :—

1st, Field-work of Slope-measurement on Left Bank.

2nd, Field-work of Discharge-measurement, &c., (occupying 2 to 3 hours.)

3rd, Field-work of Slope-measurement on Right Bank.

Thus the Field-work of the Slope-measurements on opposite Banks *was separated by 2 or 3 hours' interval.* The following is an Abstract of the Results, the details of which will be found in Sub-Column S of the Tables and Series quoted. The state

of the wind at the time of the Slope-measurement on either bank is also given in the Tables quoted.

SITE.	Table.	Serial No.	Number of Cases.	Highest Discrepancy of Slopes [of opposite banks]. L > R	R > L	Abbreviations.
15th Mile, New, ..	XLIX	196, 197	8	..	25 in 238	L > R means Left Bank Slope greater than Right Bank Slope. R > L means Right Bank Slope greater than Left Bank Slope.
Solání Embankt. Main,	LXXX	158, 159	15	47 in 265	41 in 255	
SolániTwinAqueducts,	LXXX	105, 101 112, 108	15	82 in 225	10 in 200	
Belra, ...	L, LI	201-206	49	24 in 208	150 in 330	
Jaolí, ...	LII, LIII	211-217	49	23 in 158	17 in 155	
Kamhera, ...	LIV, LV	221-225	50	89 in 301	14 in 308	

The number of Experiments reported above is very large (181): it is unfortunate of course for the purpose in hand that the measurements on opposite banks were separated by the long interval of 2 or 3 hours. Two additional Experiments are, however, available more favorable for the purpose, the measurements on the right bank having been done within three minutes of those on the left bank. The Details are given in Tab. LXXXI, *q. v.*

[Simultaneous work was impossible with the Staff (2 Observers) available: the interval of 3 minutes is simply the time taken for the Observers to cross from left to right bank].

6a. *Conclusions.*—From all this evidence, it is clear that—

"Non-simultaneous Measurements of the Local Surface-Slope of opposite banks (at same Site) are liable to be very unequal, even when not separated by more than 2 or 3 minutes' interval",..(12).

It may also be inferred as *very probable* (from the above, and from the general Unsteady state of motion of the water) that—

"The real Surface-Slopes of opposite Banks are not generally equal",........(13).

Other Experiments. The same Conclusion (as to inequality of the Surface-Slope on opposite banks) appears from Experiments on many Rivers as follows:—

Rhine Commission, '67, 4 cases, } quoted at pp. 524, 528 of Van Nos-
Elbe Experiments, '72—'74, 1 case, } trand's Maga., Vol. XVI of '77.
Lake River Report, '70, 9 cases, Report of 1870, pp. 569, 570.

It is not quite clear from the descriptions of any of the above (given in the Works quoted) whether the water-level determinations at *all* the four Slope-Points concerned in each pair of Slope-measurements (one for each bank) were simultaneous or not.

From these Results two practical Conclusions may be drawn—

"The Local Surf.-Slope should be deduced from measurements *on both banks*",(14).

"The measurements on opposite banks should be *simultaneous*",................ (15).

[It may be objected that neither of these Conditions have been fulfilled in the present Experiments. This forms of course a real objection to these Slope-measurements. All that can be said is that the Results are the best that could be readily got under the circumstances. Much of the work in the Roorkee Reach had already been executed with Slope-measurements on one bank only before the desirability of taking the measurements on both banks was fully recognized.

It was then thought better to adhere to the old system *for all the systematic work at the older Sites* (Fifteenth Mile Old Site, Solání Embankment Main Site, and Twin Solání Aqueducts) for the sake of preserving the comparability of the old and new work thereat. Again, simultaneity of the Slope-Measurements on opposite banks was a practical impossibility with the available Staff: (it would have required four Observers instead of two at each Site].

7. **Surface-Slope at different Sites.**—Whilst the *simultaneous* Discharge-Measurements (in the Roorkee Reach) detailed in Chap. XXI were in progress, the opportunity was taken of having the Slope-measurements connected therewith *effected simultaneously* (for each bank separately) at each Site. Great attention was paid to the simultaneity of the water-level determinations at the several Slope-points, thus—

Field-work. The whole of the Observers knelt down over the water's edge at their several Slope-points (along one bank) *at same time* as communicated by signal, and registered the highest and lowest water-level occurring thereat within say the next half minute. Thus the Average Free Level at all the Slope-points (of one Bank) was obtained in about the same half minute. The communication by signal between the Fifteenth Mile and Solání Sites (the banks of which are not visible from one another) was done from the centre of Mahewar Bridge (*see* Pl. I). The two Solání Sites are freely visible from one another, so that communication by signal was easy.

The Results are shown in Tab. LXXXII with certain additional data. The arrangement of this Table is as follows :—

Each line shows a SET of various data collected at about the same time at each of the Sites noted. In every case the Slope-measurements in the same line are *strictly simultaneous* (as above described). The remaining data of each line, viz., Gauge-Reading and Wind at each Site are only very roughly so, but near enough for the present purpose.

The Gauge-Readings are given here merely to show that the Experiments in question cover a tolerable range of water-level (1′·35, 2′·47, 2′·48 at the 3 Sites) sufficient to admit of drawing general Conclusions. The entry of "Variation of Gauge" shows the variation of the water-level at each Site during the period (2 to 4 hours) occupied by the Discharge-Measurement corresponding : this quantity gives a measure of the unsteadiness of the water. The state of the Wind is given merely as an indication of the varying difficulty of determining the water-level at each Site.

[The Gauge-Readings given are those taken about the time of the several Slope-measurements, *i.e.*, either *just before* or *just after* the several Discharge-Measurements corresponding, and therefore do not agree exactly with those given (for the

velocity-work)* in the general Tables. The Wind entries of same line also will seldom agree, being non-synchronous measurements of Wind at Sites with different exposure].

7a. *Conclusions.*—It may be at once inferred from these Results (as might indeed have been foreseen) that—

"The Local Surface-Slope differs at the same instant at different parts of the same Reach",...(16).

Again, from the occasional marked differences in the change of water-level at the different Sites, the existence of long waves may be suspected, though of course the want of simultaneity of the Gauge-Readings on which these differences depend throws some uncertainty on this.

8. Surface-Slope Record.—Arrangements for Surface-Slope measurement, in concert with Discharge-measurements, were made early in 1876, with the primary object of comparing the two independent values of the Mean Velocity derived from the formulæ—

$$\mathbf{V} = \text{Discharge} \div \text{Area, } i.e., \text{D} \div \text{A}, \quad \text{and } \mathbf{V} = 100\,\text{C}\,.\,\sqrt{\text{RS}}, \ldots\ldots\ldots \text{(17).}$$

8a. *Slope Record imperfect.*—The Surface-Slope Measurements were at first done *only when the air was quite calm*; it being supposed that the delicacy of the observation was such that it was useless unless done in calm air. The number of opportunities of perfectly calm air were, however, found to be so few, that after a time (from November 1877) the Surface-Slope measurement was made in general at least once a day whenever the Discharge was taken, *recording the state of the Wind* at the same time.

[Thus the record of Surface-Slopes accompanying Discharge-Measurements is imperfect in all the early years 1875-1877 as shown below.

SITE.		YEAR.	DISCHARGE-MEASUREMENTS.	SLOPE-MEASUREMENTS.			
				Left Bank.	Right Bank.	Both Banks.	Standard Bank.
15th Mile	New Site, ...	1878-79	4	4	8	8	Both
15th Mile	Old Site, ...	1878	13	12	0	0	Left
Solani Sites	Embankment Main, ...	1876-79	153	92	15	15	Left
Solani Sites	Left Aqueduct, ...	1875-79	45	15	0	15	Left
Solani Sites	Right Aqueduct, ...	1876-79	174	0	78		Right
Solani Sites	„ „ Left Aqued. closed,	1876-78	12	0	10	0	Right
Belra, ...		1879	53	50	49	49	Both
Jaolí, ...		1879	55	51	49	49	Both
Kamhera, ...		1879	56	50	50	50	Both

* these being all "Mean Gauge-Readings", (Ch. V, 12.)

It will be seen that in all the earlier Experiments *at the older Sites in the Roorkee Reach*, the Slope-Measurements were made *on one Bank only*. On a few special occasions they were made on both banks; these are recorded in special Tables LXXX, LXXXI, and discussed in Art. 6, but throughout the rest of the Tables only one of these has been used, viz., *that of the Standard Bank*, so as to preserve the comparability of Results throughout these Tables].

8b. WIND ENTRIES.—The state of the Wind *at the time of the Slope-Measurements* will be found in Col. 5 of Tab. LVIII to LXIX: there being a single entry for Slope-measurements on only one Bank (Tab. LVIII to LXV), and two entries for Slope-measurements on both banks (Tab. LXV to LXIX).

[The Wind-entries in all the Detailed Velocity-Tables VII to LV, are everywhere those *proper to the velocity-work*, those proper to the Slope-measurements not being there given for want of space].

8c. *Other Surface-Slopes.*—Except as above—*i. e.*, except when Discharges were being taken—Surface-Slope Measurements were *not made with any regularity* throughout the greater part of the Experiments.

A great many were indeed made (at the suggestion of certain* hydraulicians) chiefly in calm weather, with all the other sorts of velocity-work, but in most of the Series, the record is very imperfect, *see* Col. 3 of Tab. VII to XXXIII, and the state of the Wind (at the time of Slope-measurement) has not been printed.

9. **Surface-Fall.**—After the Experiments had been a long time in progress, it was found that the state of the Surface-Slope appeared to exercise a far greater influence on the Velocities and Discharges than had been expected. The imperfection (above explained) of the Surface-Slope Record in all the early Experiments prevented this being thoroughly traced out. But a means fortunately existed of supplementing this by a quantity nearly related to the above, viz., the Total Surface-Fall in the Upper Half and Lower Half of each Reach.

9a. HEAD-GAUGE, TAIL-GAUGE.—In each Reach there is usually a permanent Gauge a little below the Head of the Reach, and another a little above the Tail of the Reach. These Gauges will be called for shortness the HEAD-GAUGE and TAIL-GAUGE of the Reach. The Readings on these Gauges show the water-level shortly after entry into, and just before exit from, the Reach, and serve to indicate *roughly* the state of supply into, and of withdrawal from, the Reach.

[These Gauges were read some *once*, some *two or three times a day* (for Canal purposes), and the Readings were filed in the Canal Records.

The value of this additional information was first perceived about end of 1876. For the work already past, viz., from 1875 to end of 1876 (and, therefore, affecting the Roorkee Reach only), the Gauge-readings were taken from the old records of the Canal: after that date they were communicated monthly by the Canal authorities.

The water-levels at these Gauges being thus known, give, together with the water-level at the Experimental Site, the means of finding the TOTAL SURFACE-FALL in

* Mr. H. Bazin, and Genl. Th. Ellis.

the portions of the Reach above and below the Experimental Site, and also in the whole Reach.

These will be denoted by the Symbols F_1, F_2, F_3, F, thus—

F_1 = Surface-Fall in Upper Sub-Reach, *i.e.*, from Head-Gauge to Experl. Site, (except in case of Solání Embankment Minor Sites, *v. inf.*)

F_2 = Surface-Fall in Middle Sub-Reach, *i.e.*, from Experl. Site to principal Gauge next below, (used for Roorkee Reach only.)

F_3 = Surface-Fall in Lower Sub-Reach, *i.e.*, either from the Experl. Site, or from the principal Gauge next below, to the Tail Gauge.

F = Total Surface-Fall in the Whole Reach, *i. e.*, from Head to Tail.

The details for each Site are shown in Table below—

REACH.	HEAD-WORKS to TAIL-WORKS.	Length of Reach. M. Ft.	EXPERIMENTAL SITE.	SUB-REACHES from	SUB-REACHES to	Length of Sub-Reach M. Ft.	Surface-Fall Symbol.	Fall of Bed [original].
ROORKEE.	Dhanaurí Regulator, to Asafnagar Falls,	9, 3562	15th Mile, [New Site],	Head-Gauge,	Experl. Site,	1, 4891	F_1	?2·3
				Experl. Site,	Solání Aqueduct,	3, 434	F_2	4·3
				Solání Aqueduct,	Tail-Gauge,	4, 3386	F_3	4·9
			15th Mile, [Old Site],	Head-Gauge,	Experl. Site,	1, 4921	F_1	?2·3
				Experl. Site,	Solání Aqueduct,	3, 404	F_2	?4·3
				Solání Aqueduct,	Tail-Gauge,	4, 3386	F_3	4·9
			Solání Embankment [Minor Sites],	Head-Gauge,	Solání Aqueduct,	5, 45	F_1	6·6
				Solání Aqueduct,	Tail-Gauge,	4, 3386	F_3	4·9
			Solání Embankment [Main Site],	Head-Gauge,	Experl. Site,	4, 90	F_1	5·4
				Experl. Site,	Solání Aqueduct,	.. 5235	F_2	1·2
				Solání Aqueduct,	Tail-Gauge,	4, 3386	F_3	4·9
			Solání, Aqueducts,	Head-Gauge,	Experl. Site,	5, 45	F_1	6·6
				Experl. Site,	Tail-Gauge,	4, 3386	F_3	4·9
BELRA.	Nirgájní Falls, to Jaolí Falls,	5, 4099	Belra,	Head-Gauge, (at tail of lock channel)	Experl. Site,	.. 5073	F_1	?·9
				Experl. Site,	Tail-Gauge,	3, 4908	F_3	?5·5
JAOLI.	Jaolí Regulator Falls, to Chitaura Falls,	5, 2425	Jaolí,	Head Gauge,	Experl. Site,	.. 1952	F_1	?·3
				Experl. Site,	Tail-Gauge,	5, 152	F_3	?6·0
KAMHERA.	Anupshahr Branch Head-Works, to Churiyála Falls,	10, 2508	Kamhera,	Head-Gauge,	Experl. Site,	2, 642	F_1	?8·2
				Experl. Site,	Tail-Gauge,	8, 1822	F_3	?10·1

The several Surface-Falls (F_1, F_2, F_3) are entered for every SET of Field-work in Col. 3 of Tab. VII to LVII, and in Col. 2 of Tab. LVIII to LXIX.

It is obvious that—

$$\left.\begin{array}{l} F = F_1 + F_2 + F_3, \text{ at those Sites where } F_2 \text{ is entered,} \\ F = F_1 + F_3, \text{ at all other Sites,} \end{array}\right\} \quad \text{.................} (18).$$

10. Control.—The Control exercised over the Supply into, and Withdrawal from, each Reach is what ultimately determines the Surface-Fall within the Reach; the variation of the Surface-Fall cannot in fact be understood at all without studying the state of the CONTROL.

10a. CONTROL AT HEAD.—The Control exercised at the Head of each Reach exerts little influence over the mode of passage of the water through the Reach, *i.e.*, over the Fall of the water-surface: for, however admitted into the Reach, the water rapidly spreads itself out all over the Bed, so as to fill up the whole width of the channel *nearly level across*, so that the effect of Control at the Head is soon lost; and it would be unnecessary to notice it further, were it not that the *Head-Gauges are situate within its influence*, as will be discussed further on.

10b. CONTROL AT TAIL OF REACH.—The manner of Control at the Tail of the Reach has been already explained (Ch. III, 5a, b) to consist of—

1°. Increase or Decrease of the Total Area of the *Débouchure* of the Reach by (partially or wholly) opening or closing the Drop-gates of the Distributaries taking off near the Tail of the Reach.

2°. Raising of the Crest of the Falls in one or more Bays, by lowering wooden "Sleepers" on to the top of the masonry Crest of such Bays.

The latter is by far the most efficient means of controlling the mode of passage of the water through the Reach, as the raising and lowering of the Crest of the Falls decreases and increases the Fall of the water-surface in the Reach above in the most direct manner: this will appear fully below.

[The necessity for this mode of Control has been explained (Ch. III, 5b) to arise only during Low Supply (in order to raise the water-level sufficiently to force the water into the Distributaries).

The amount of such Control that can be exercised is very great, the highest water-level at the Tail of the Roorkee Reach (Asafnagar Falls) having actually occurred at time of very Low Supply, (*see* Ser. 131, 171, 172 of *Abstr. Tab. 1, and Det. Tab. XLI, XLVII].

10c. *Abnormal Readings.*—Were the Obstruction required—whether of Drop-gates or Sleepers uniformly applied across all the Bays of the Regulating Works, the water-level in each Bay would probably be equally affected. But, from the difficulty of raising the Drop-gates and Sleepers in numerous Bays, it is found practically more convenient to apply the whole Obstruction in a small number of Bays, rather than equally in all the Bays.

* Abstr. is short for Abstract, Det. for Detailed.

The Result is that the Obstruction takes the form *at any rate partly* of a CONTRACTION in width of the Waterway at the Regulating Works. In consequence of this, it follows then that—

"The water-level in the immediate neighborhood of the Regulating Works stands abnormally high when Obstruction is applied",..............................(19).

[By *abnormally high* is here meant, higher than it would if the Obstruction had taken the form of an equal lowering of the Drop-gates, or an equal raising of the Crest of the Falls throughout all the Bays alike, instead of the form of a partial Contraction of the Waterway].

It seems probable that this abnormal raising of the water-level takes effect only in the neighborhood of the Regulating Works, and is insensible at any considerable distance from them: this seems certainly the case with respect to the Head-Works, as before remarked. Unfortunately the Head- and Tail-Gauges are situated so close to the Controlling arrangements, (*see* Table below), being usually fixed on their masonry waterwings, as to be within this influence. Moreover, in consequence of the Gauges being applied on one Bank (*ibidem*), there is some uncertainty as to (the meaning of) their indications when Obstruction is applied as above, as the water-level near the Gauge is liable to be differently affected, according as different Bays (near to or far from the Gauge) are obstructed.

REACH.	HEAD-WORKS and TAIL-WORKS.	HEAD-GAUGE and TAIL-GAUGE. ["Free" means that the Gauge shows "Free water-level".] Position. Bank.	Distance.	Description.	Facility of Readings.*	Observer†	Distributaries. Number to left.	to right.	Max. Discharge.
ROORKEE.	Dhanaurí Regulator,	Left	31′ below Regulator,	Still water.	Inaccurate,	C	..	..	..
	Asafnagar (13′) Falls, (5′ raised crest).	Left	100′ above Crest,	Free, [Stone Slab].	Accurate,	C	2	2	500
BELRA.	Nirgájní (14′) Falls,	Left	4478′ below Falls,	Free, [Iron Bar].	Accurate,	C, E	..	..	..
	Jaolí (18′½) Falls, (5′½ raised crest).	Left	200′ above Crest,	Free, [Stone Slab].	Accurate,	C	2	2	440
JAOLI.	Jaolí Regulator (5′) Falls,	Left	121′ below Falls,	Free, [Wood Bar].	In rough water,	E	..	..	..
	Chitaura (12′) Falls, (4′ raised crest).	Left	200′ above Crest,	Free, [Wood Bar].	Inaccurate,	C	0	1	110
KAMHERA.	Anupshahr Branch Head-works,	Right	18′ below Head-works,	Free, [Iron Bar].	Fairly accurate,	E	..	..	..
	Churiyála (9′½) Falls, (2′½ raised crest).	Right	26′ above Crest,	Free, [Stone Slab].	Accurate,	C	1	1	125

* In this Column the terms "Accurate", "Inaccurate", imply simply that the state of the Gauge is (by construction, position, &c.) such as to admit, or not to admit, of accurate reading.

† C means "Canal Staff"; E means "Experiments' Staff".

The General Result is—

"The Head- and Tail-Gauge Readings are usually abnormally high and of somewhat uncertain value whenever *partial* Obstruction is applied (in form of contraction of the width)", ...(20).

In the present Experiments, however, the Head- and Tail-Gauges of the Roorkee Reach alone are affected in a manner worth noticing, as will be fully shown below (Art. 13a, *et seq.*), from the great range of Control that occurred therein. In the other Reaches the Experiments lasted only 3 months (Jany., Feby., and March '79), during which but *little change of Control occurred*: their case is as follows:—

Belra Reach. Head-Gauge 4478′ below Head-Works, and therefore out of their influence. The Obstruction of the Falls at the Tail occurred only in one month, (Feby. '79,) and was always small: it was always applied from the Left bank outwards, never extending right across. The Tail-Gauge, being on the Left Bank, was thus always *somewhat similarly affected* on such occasions, (always reading then somewhat *abnormally high*, see above.)

Jaoli Reach. No Obstruction applied at the Head-Works during the above period. The Obstruction of the Falls at the Tail was always of uniform height in each Bay, so that the Tail-Gauge readings probably remained "normal" (in the sense above used).

Kamhera Reach. The water was admitted through the Anupshahr Branch Canal Head-Works (Pl. III, VI) of 3 Bays. The Drop-Gates of the two Left Bays were kept permanently lowered (thus closing the Bays) to the same extent throughout the above period. More or less water was admitted into the Reach by raising or lowering the Drop-gate of the Right Bay. The Head-Gauge being on the right Bank was thus always *somewhat similarly affected,* (always reading *abnormally high,* see above.)

The Tail-Works consist of Falls of three Bays: each Bay was kept constantly obstructed by an unvarying amount, their crests being raised 1′·50, ·86′ and 1′·42 respectively, throughout the whole period. In consequence of the constant greater Obstruction in the side Bays, the Tail-Gauge was *always similarly affected,* (always reading somewhat *abnormally high,* see above.)

It will be seen that the Head- and Tail-Gauges of these three Reaches, when affected at all abnormally by partial Obstruction of the Regulating Works, were in each case *similarly affected,* so that the affection is not of so much importance.

11. Surface-Falls imperfect.—For various reasons the Surface-Falls in the Upper and Lower Sub-Reaches, obtained as above described, (from the Head and Tail-Gauges,) have not the same value as the Local Slopes: these are—

i. Abnormal Gauge-Readings (caused by Canal-control).
ii. Inaccuracy of these Gauge-Readings.
iii. Non-simultaneity of the Gauge-Readings.

11a. *Abnormal Gauge-Readings.*—The occasional abnormal height and comparative uncertainty of the Readings of the Head- and Tail-Gauges explained above as caused by the practice of unsymmetric obstruction, obviously affects the Surface-Falls of the Upper and Lower Sub-Reaches (F_1, F_2) as follows, (observing that the Gauge-Readings at the Experimental Sites are not thus affected):—

"The Surface-Fall measurement in the Upper Sub-Reach (F_1) is liable to be *over-estimated* to an uncertain extent at low water whenever the Head-Works are partially (unsymmetrically) obstructed", ...(21).

"The Surface-Fall measurement in the Lower Sub-Reach (F_3) is liable to be *under-estimated* to an uncertain extent at low water whenever the Tail-Works are partially (unsymmetrically) obstructed", ...(22).

[The Surface-Fall of the Middle Sub-Reach (F_2) is of course unaffected hereby].

11b. *Inaccurate Gauge-Readings.*—For various reasons the readings of the Head- and Tail-Gauges are not nearly so accurate as those of the Gauges at the Experimental Sites—

1°. Some few of the Gauges are situate in troubled water which oscillates so much and so irregularly, that the Water-Level (taken as the mean of the highest maximum and lowest minimum) *cannot be precisely defined.*

2°. Most of these Gauges were read by the petty subordinate Staff of the Canal, so that the readings *cannot be depended on closer than the nearest tenth* of a foot.

Great accuracy in these readings is, however, by no means essential, as the Surface-Falls in the long distances in question are comparatively large quantities, so that small errors in the Gauge-readings do not seriously affect the Results.

11c. *Non-Simultaneous Gauge-Readings.*—The Readings of the several Gauges (viz., Head, Intermediate, and Tail) were *not in any case simultaneous*, so that the Surface-Falls (F_1, F_2, F_3) are liable to be abnormally affected if any considerable change of water-level took place in the interval. This source of uncertainty is greatest in the Upper and Lower Surface-Falls (F_1 and F_3) because most of the Head- and Tail-Gauges (viz., all those read by the Canal Staff, *see* Tab. of Art. 10c) were read only at certain times of day (sometimes only once a day), often not coinciding closely with the Experiments' Field-work.

[Small variations of water-level would be of little practical importance, for the reason given above that great accuracy is not essential. The cases in which the Results are likely to be abnormal from this cause (chiefly at low water) can be easily recognized in the Tables, as being those in which the "Variation" of water-level at the Site itself is considerable].

Notwithstanding these* defects, the Surface-Falls in question are a valuable record, in helping to trace the cause of very different velocities and Discharges occurring at one Site *with same water-level*, when the Local Slope record is wanting, and also in confirming the indications of the latter when existing.

12. Effect of Control.—In order to study the effect of the Control on the Surface-Fall, it might seem necessary to give the state of the Controlling arrangements for every Experiment wherein the Surface-Fall is registered. To have done this would, however, have involved a great extension of the printed Tables (the Surface-Fall being given for *every* Set of systematic velocity-work throughout the Tables, Art. 9a). It

* The entries $F_3 = -{\cdot}38$, (Ser. 126); $F_3 = {\cdot}00$ (Ser. 133 & 171), are probably *mistakes* arising from the causes above noticed.

has been thought sufficient to limit the inquiry to those SETS of Mean Velocity-work along with which a Slope-Measurement was made, (Art. 8a).

[These Sets are by far the most interesting in the whole Work; and they are ample for the purpose, as they cover the whole Range of water-level occurring in the Experiments, which—in the case of the Roorkee Reach—was very large (from 10′ to 0′·7 at the Solání Aqueduct Gauge), and include great changes in the state of Control].

13. Control, EXHIBITION OF.—The following system of exhibiting the state of the Control at the Head- and Tail-Works in a compendious manner has been adopted, and is used throughout the "Comparison* Tables" (LVIII—LXIX, and 20—22).

13a. CONTROL AT HEAD, *Roorkee Reach.*—The water is admitted into the Reach through the Dhanaurí Regulating Works (Pl. I, *Fig.* 1) of 10 Bays, each Bay of which can be (wholly or partially) closed from overhead by Drop-Gates, (one to each Bay.) The surplus water in the Reach above is allowed to escape (if required) out of the Canal into the Rátmú Torrent through the Dhanaurí Dam (Pl. I, *Fig.* 1) of 57 Openings (47 large and 10 small: the latter are—from the form of their bed—styled Ogees); these are ordinarily kept closed (to prevent the escape of the Canal-water) by Flood-Gates, one to each Opening, *i.e.*, 47 large Flood-Gates, and 10 small Flood-Gates (in the Ogees).

A statement of the total numbers of large and of small Flood-Gates *open* in the Dhanaurí Dam, and of Drop-Gates *closed* in the Dhanaurí Regulator on each day, was supplied †by the Canal Staff. These numbers are entered in Col. 2 of Tab. LVIII—LXV in the Sub-Columns headed—

Gates open in Dam, Ogees open in Dam. Gates closed in Regulator.

These figures serve to indicate in a rough way the chance of the Head-Gauge being affected as described above, and, therefore, also of the Surface-Fall Measurement being abnormally high, (Art. 11a.) It is clear that the closure of the Dhanaurí Regulator Gates is the more important element affecting the Head-Gauge (from its situation in the left waterwing thereof, Art. 10c).

13b. *Other Reaches.*—For the reasons given in Art. 10c, it has not been thought worth while giving any record of the state of Control at Head of the other Reaches (Belra, Jaolí and Kamhera).

13c. CONTROL AT TAIL.—This has been explained to consist of two items—

1°. Withdrawal of water by the Distributaries.

2°. Partial Obstruction of (some bays of) the Falls at Tail.

13d. *Withdrawal by Distributaries,* (Q).—The Distributaries leading out of each Reach are shown in the Plans of the several Reaches (Pl. I, & III—VI): their number, and the maximum Discharge of each, are shown in the Tab. of Art. 10c, *q.v.*

The most convenient mode of exhibiting compendiously their effect upon the state of the water in each Reach seemed to be to give simply the *Total volume withdrawn* on each occasion by the whole of the Distributaries in each Reach. With this view the Gauge-Reading (or in some cases the actual Depth near the Gauge) of the early

* so called from showing several values of Mean Velocity, collected for ready comparison, to be discussed in Ch. XX.

† Once a day (at early morning) in 1875, '76; thrice a day (morning, noon, and sunset) in 1877 '78, '79.

morning of each day for each of the Distributaries concerned was supplied by the Canal Staff. From this the Discharge passing the Gauge was taken out from the official Discharge-Tables in use on the Canal. The sum of these Discharges of all the Distributaries in each Reach is the Total Volume (measured in cubic feet per second withdrawn by the Distributaries of the Reach. This is what is entered in Col. 2 of the "Comparison Tables" LVIII—LXIX, and 20—22 as the —

"Withdrawal by Distributaries", (in cub. ft. per sec.,) denoted by Q.

The ratio of this quantity to the Cubic Discharge (D) of the Reach, (Col. 3 of same Tables) will give an idea of the effect of the Distributaries on the Surface-Fall.

13e. *Average Obstruction at Tail* (k).—The manner of Obstruction at the Tail of each Reach, by *temporarily raising the Crest in one or more Bays* of the Falls, has been already explained (Art. 10b). With the view of tracing the effect of this on the Surface-Fall, the height of the Obstruction across each Bay (*i.e.*, the height of the Sleepers placed on the Crest of each Bay) of the Falls at Tail of each Reach at about 7 A.M. of each day was supplied by the Canal Staff. To exhibit this in a compendious manner, it was thought, however, sufficient to print only what may be called the Average Height of Obstruction, or shortly AVERAGE OBSTRUCTION, computed as follows :—

$$\text{Average Obstruction } (k) = \frac{\text{Total Obstructed Area}}{\text{Total Waterway}},$$

$$= \frac{\text{Sum of heights of Obstruction in each Bay}}{\text{Number of Bays}} \quad \text{......(23)},$$

the Bays in each system of Falls being of equal width.

This is the quantity entered in Col. 2 of Tab. LVIII—LXIX, and 20—22.

13f. *Obstruction Record imperfect* (in Roorkee Reach).—The value of this last quantity (k) in tracing out the causes of the very great variations of Surface-Fall which occurred in the Roorkee Reach was first perceived about end of 1877, from which time a regular record of the state of Obstruction at the Tail of this Reach was supplied by the Canal Staff. For the time preceding, such record was supplied as was available: it is unfortunately very imperfect from the fact that the record was not regularly preserved.

[It has been, however, possible to supply it (with almost absolute certainty) for all cases of High Supply even for that earlier period, because such Obstruction is never applied in times of High Supply, so that for all such cases of High Supply the Obstruction was (as far as can now be recalled) almost certainly zero ($k = \cdot 00$). To indicate, however, that the entry is to some extent conjectural, (*i.e.*, is not a real observation,) the entry is printed with a query, (thus $k = ?\cdot 00$) in all these cases.

In a few other cases where there was evidence available, but only in a somewhat imperfect form, the entries have been printed with a query thus (?4·30) to indicate that they are somewhat conjectural.

These queried entries are by no means to be rejected, as they are all almost certainly correct].

13g. *Detail imperfect.*—As far as the Surface-Fall of the Lower Sub-Reach (of all the Reaches) is concerned, the detail given (viz., the values of Q, k) would probably have been nearly perfect for the purpose of showing the effect of the Control at Tail, except for the position of the Tail-Gauge close to the controlling arrange-

ments (*see* Tab. of Art. 10c). Again, as far as the Surface-Fall of the Roorkee Upper Sub-Reach is concerned, the details given would not be required at all (*see* Art. 10a), except for the position of the Head-Gauge close to the controlling arrangements.

The detail actually given cannot be said to afford more than a rough idea of the effect of the Control in causing abnormally high readings on the Head- and Tail-Gauges. But this is all that is really required. To have shown it at all fully would have required the showing the amount of Obstruction applied *in each Bay separately*, viz.—

Roorkee Reach: Head-Works, 10 Bays; Tail-Works, 8 Bays.
Belra and Jaoli Reaches: Tail-Works, each 8 Bays.
Kamhera Reach: Head-Works, 3 Bays; Tail-Works, 3 Bays.

This would evidently have involved an immense increase of the printed Tables. The practical advantage gained would be doubtful, as it would not enable any correction to be applied to the abnormal Gauge-Readings to reduce them to normal values.

As already remarked (Art. 10c), the abnormal state of the Head- and Tail-Gauges is not of much importance in the three latter Reaches.

14. Surface-Fall and -Slope, DIAGRAMS, (Pl. XXI, XXII, and XLIV—XLIX).—Several Diagrams prepared primarily with the view of showing the dependence of Discharge and Velocity on various elements, (such as Depth, *Surface-Fall*, *Surface-Slope*, Control at Tail, Wind, &c.,) serve also equally well for showing the dependence of the Surface-Fall and Local Surface-Slope on the remaining elements Depth at Site, Control at Tail, Wind, &c.

Thus Pl. XXI, XXII show the MEAN SURFACE-FALLS (F_1, F_2, F_3) plotted as ordinates to the MEAN DEPTHS (H) on the Experimental Vertical taken as abscissæ for all the Subsurface Velocity-work upon a central vertical, *i.e.*, *for all the Series* 1—28, (Tab. 3, 4,) together with the MEAN WIND plotted as explained in Ch. V, 21d.

Again, Pl. XLIV shows the MEAN SURFACE-FALLS (F_1, F_3) plotted as ordinates to the MEAN HYDRAULIC MEAN DEPTHS (R) at the Experimental Sites, taken as abscissæ for *all the Surface-Velocity Series* 51 to 59, (Tab. 13,) together with the MEAN WIND plotted as explained in Ch. V, 21d.

Pl. XLV to XLVIII show the MEAN SURFACE-FALLS (F_1, F_2, F_3), LOCAL SURFACE-SLOPE (S), MEAN WITHDRAWAL BY DISTRIBUTARIES (Q), and MEAN AVERAGE OBSTRUCTION (k) at Tail, plotted as ordinates to the MEAN HYDRAULIC MEAN DEPTHS (R) at the Experimental Sites, taken as abscissæ for most of the Mean Velocity Work, viz., *for each of the Mean Velocity Series* 101 to 225* contained in Tab. 20—22, together with the MEAN WIND plotted as explained in Ch. V, 21d.

Lastly, Pl. XLIX shows the SURFACE-FALLS (F_1, F_3), LOCAL SURFACE-SLOPE (S), WITHDRAWAL BY DISTRIBUTARIES (Q), and AVERAGE OBSTRUCTION (k) at Tail, plotted as ordinates to the HYDRAULIC MEAN DEPTH (R) at the Kamhera Site, taken as abscissæ, *separately for each day's Mean Velocity work* at that Site, taken from Ser. 221 to 225, (Tab. LXIX.)

* A selection as explained hereafter from those bearing same Serial Nos. in Tab. XXXIV to LV.

[Thus Pl. XXI, XXII, and XLIV to XLVIII show only the ***Mean Results of the several Series*** indicated, (one ordinate being allotted to each Series.) Pl. XLIX shows the ***Results of each day's work*** separately, (one ordinate being allotted ***to each day's work***, *i.e.*, usually one SET, or in a few cases—where two Sets were done in one day—the Mean Result of two Sets)].

15. Surface-Gradient, Variation.—From these Diagrams the following broad Conclusions may be at once drawn: the evidence will appear in discussing the detailed Conclusions:—

"The Surface-Fall (in a long distance) depends chiefly on two elements, viz., 1°, Depth of water; 2°, Obstruction at Tail", ...(24).

"The variation of Surface-Fall (in a long distance) accompanying change of depth is quite different in the Upper and Lower Sub-Reaches",..................(25).

"The Local Surface-Slope seems to depend jointly on the Surface-Falls in both the Upper and Lower Sub-Reach",...(26).

A great difference might of course be expected in the Upper and Lower Sub-Reaches on account of the proximity of the latter to the Obstructed Falls at the Tail.

15a. SURFACE-FALL VARIATION.—Passing now to details, the following Conclusions (Nos. 1°—9°) may be drawn as to the variation of the Surface-Fall at different parts of a Reach:—

1°,—"The Surface-Fall in the Upper Sub-Reach (F_1) is at most of the Sites generally (except at low water) less than the (original) Fall of Bed",.......... (27*a*).

[This is best seen by glancing down the Sub-Column of F_1 in the Abstract Tables **3, 4, 13—18, & 20—22**: the values of F_1 will be seen to be at the Roorkee Reach and Kamhera Sites generally (*i.e.*, except at low water) less than the original Fall of the Bed corresponding. This is no doubt due to the permanent Obstruction (in the slope of the raised Crest of the Falls) at the Tail of each Reach. As it does not obtain in the case* of the Belra and Jaolí Sites, it is no doubt a local peculiarity depending on the amount of permanent Obstruction at the Tail of Reach.

To save further reference, the (original) Fall of Bed in the Upper Sub-Reach for each Site is here re-quoted from the Table of Art 9a,—

15th Mile Sites 2·3, Solání Embankment Main Site 5·4, Aqueduct Sites 6·6, Belra Site ·9, Jaolí Site ·3, Kamhera Site 3·2].

2°,—"The Surface-Fall in the Upper Sub-Reach (F_1) appears (in absence of other influences) at most of the Sites to *increase slowly with decrease of depth*",...(27*b*),

[*see* Pl. XXI, XLVI, XLVII, wherein it is well marked on the whole: the change is, however, so small, that it is liable to be masked or even reversed by the effect of other causes; thus increased Obstruction at the Tail (*k*) appears to account for the opposite change at the Belra and Jaolí Sites, (Pl. XLVIII)].

3°,—"At very low water the Surface-Fall in the Upper Sub-Reach (F_1) appears *liable to equal and even exceed the Fall of Bed* (even in those cases where it is less at high water)," ....(27*c*),

[*see* Ser. 20, 124, 125, 127, 180, 181, in Tab. **3, 15, 17**, and Pl. XXI, XLVI, XLVII. This effect is probably enhanced *abnormally* by abnormally high Head Gauge-Readings due to *partial closure* (contraction of width) of the Head-Works, as

* The exceptions are numerous; this is best seen in the Detailed Tables L—LIII.

most of the known cases of such partial closure (of Gates in Dhanauri Regulator) are accompanied by *high values* of F_1; *see* Ser. 122—127, 132—139, 176, 180, 181, in Tab. 20, 21.

4°,—"The Surface-Fall in the Middle Sub-Reach (F_2) does not change in any obviously regular manner with change of depth", [*see* Pl. XXII, XLVII],...(27*d*).

5°,—"The Surface-Fall in the Lower Sub-Reach (F_3) is *greatest in deepest water*, and *decreases rapidly* (in absence of other influences) *and pretty regularly with decrease of depth*",..(27*e*).

[This is obvious throughout all the Pl. XXI, XXII, and XLIV—XLIX].

6°,—"The *larger changes* of Surface-Fall in the Upper and Lower Sub-Reaches (F_1, F_3) are usually *of similar character*",..(27*f*).

[This is shown by the *general concurrence of saliences and depressions* in the curves of F_1, F_3, in all these Plates, especially in Pl. XLVI, XLVII. In consequence of decrease in depth being accompanied (*v. supra*) by change of opposite kinds in F_1, F_3, the similarity of change noticed *does not involve simultaneous increase* or *decrease* of F_1, F_3. This general concurrence (of saliences and depressions) is liable to be masked by various causes partly noted below, especially at low water].

7°,—"The variation of Surface-Fall in the middle Sub-Reach (F_2) does not seem to be generally concurrent with that in the Upper and Lower Sub-Reaches (F_1, F_3)", ...(27*g*),

[*see* Pl. XXII, XLVII; the saliences and depressions of the curve of F_2 cannot be said to be generally concurrent with those in the curves of F_1, F_3].

8°,—"Obstruction at the Tail decreases the Surface-Falls (F_1, F_2, F_3) and also the Local Surface-Slope (S) generally (in absence of other influences)",...... (28*a*).

[This is obvious throughout Pl. XLV to XLVIII, *nearly all high values of k being attended with low values of* F_1, F_2, F_3, S. This is so regular on the whole, that it seems probable that many of the apparent exceptions in the Diagrams are due to the imperfect record of the Obstruction available, thus—

Pl. XLVI. The only exceptions are Ser. 118, 119 (R = 5·48), 121 where the high values of *k* plotted do not seem to have produced sufficient depression in the curves of F_1, F_3, S: but on reference to the details (Tab. LX) it will be seen that both mean values of *k* are derived from imperfect data.

Pl. XLVII. In this Plate in tracing the effect of the Obstruction at Tail, it is obviously necessary to exclude the ordinates on which the value of *k* is unknown, viz., Ser. 179, 177, 175, 174, 170, 169, 166, 165, 163].

9°,—"This effect (decrease of Surface-Fall due to Obstruction) is much the greatest in the Lower Sub-Reach", [*see* Pl. XLV—XLVIII, *passim*],..........(28*b*).

15b. Surface-Slope Variation, (Pl. XLV—XLIX).—The variation of Local Surface-Slope is much more difficult to account for than that of the Surface-Falls in the long stretches (Sub-Reaches). The following general Conclusions may be drawn from the Plates quoted:—

"The Local Surface-Slope (S) does not change in any obviously regular manner with change of depth", [*see* Pl. XLV to XLIX],....................................(29*a*).

"It seems to partake of the variation of the Surface-Falls in the several Sub-Reaches, sometimes following one, sometimes another",..........................(29*b*).

[At the Soláni Right Aqueduct the Slope-variation seems to follow the Upper

Surface-Fall (F_1) in deep water, and the Lower Surface-Fall (F_3) in shallow water (compare the curves of F_1, F_3, S, Pl. XLVI). Again, at the Solání Embankment Main Site, it seems to follow the Middle Surface-Fall in deep water, and the Lower Surface-Fall to some extent in shallow water].

"In its larger features it seems to depend *chiefly on the amount of Obstruction at Tail*, (decreasing rapidly with increase of Obstruction)," ....(29c), [compare with Results (28a, b) above].

The occasional complete disagreement of the curves of F_2 and S (which may be both considered *local* Curves) with each other and with the curves of F_1, F_3 (which may be considered *general* Curves), seems to indicate the *passage of waves* down the Reach. Such a state of things would of course temporarily mask the interdependence of these quantities, [*see* Pl. XLVII, Ser. 156, 176, 180.]

On the whole it may be said that though the Upper and Lower Surface-Falls together afford a good indication of the state of the Surface in the Reach generally, they *by no means suffice to indicate the Local Surface-Slope* at any particular Site.

[This might have been expected from Result (16), Art. 7a].

15c. *Withdrawal by Distributaries.*—It might be expected pretty confidently *à priori* that an increase of Withdrawal in the Distributaries (taking off the Canal near the Tail of each Reach) would *cæteris paribus* increase the Surface-Fall, at any rate in the Lower Sub-Reach. This effect is, however, very obscure in the Diagrams (Pl. XLV—XLIX), and especially in the Lower Sub-Reach: this is probably due to the fact that—

1°. At high water the maximum Withdrawal by Distributaries (Q) is only a small fraction of the Total Cubic Discharge (D) passing through the Reach, and, therefore, produces a proportionately small effect.

2°. At low water when the Withdrawal by Distributaries (Q) is a much larger, and occasionally an important, fraction of the Total Cubic Discharge (D) through the Reach, the effect seems to be wholly masked by the occurrence of the (far more effective) Obstruction at Tail of Reach.

[*See* Tab. LXI. In Ser. 131 $Q \div D > \frac{1}{5}$, whilst in the remaining Ser. 132 to 139 at this Site $Q \div D$ lies between $\frac{1}{6}$ and $\frac{1}{15}$.

See also Tab. LXIV. In Ser. 171, 173, $Q \div D > \frac{1}{4}$ and $\frac{1}{4}$ respectively, whilst in Ser. 174, 176, $Q \div D < \frac{1}{5}$, and in all other neighboring Series much less.

In all the more marked cases of relatively large Withdrawal by Distributaries (Ser. 131, 171, 173), the effect on (*i.e.*, expected enhancement of) the Surface-Falls and -Slope is *quite masked by the occurrence of high Obstruction* at Tail of Reach].

Although the effect is generally quite obscure, there is in some of the Plates a marked agreement between one or other of the curves of F_1, F_3, and Q, *at high water* (in absence of variation of Obstruction at Tail of Reach) in the general concurrence of the saliences and depressions upon the same ordinates.

The variation of the Withdrawal in the Distributaries seems here to be *the* efficient cause of the variation of the Surface-Fall: high and low Withdrawal (Q) producing high and low Surface-Fall (F_1, F_3) respectively.

[*See* Pl. XLVI, Ser. 108 to 117. No Obstruction at Tail of Reach. Here the curves of F_1, Q agree in their several irregularities throughout the range indicated; but the curve of F_3 is strikingly different.

Also Pl. XLIX, *passim*. Obstruction at Tail of Reach constant throughout. Here the curves of F_3, Q agree generally in their several irregularities throughout the whole range: the marked exceptions are few, viz., where R = 4'·40, 4'·13, 4'·09, 4'·06, (or 4'·05,) & 4'·02; it must be admitted, however, that this includes most of the more marked irregularities].

16. Free Surface, DIAGRAMS, (Pl. VIII, IX).—These Diagrams are intended to show the *figure assumed* by the Free Surface of the water *along a Reach* with various depths of water, and various states of Control (as shown by the Average Obstruction at Tail of Reach). The Plates show the Results for the Roorkee and Jaolí Reaches only.

[The Roorkee Reach Results (Pl. VIII) are by far the most instructive from the great range both of water-level and of Control that occurred therein. The similar Diagrams for the Belra and Kamhera Reaches have not been published, simply because they were found to contain nothing instructive, not sufficiently illustrated by the Roorkee and Jaolí Reach Results].

16a. *Explanation.*—Each Plate shows a longitudinal section of the Reach with details nearly as in the general Pl. I, V described in Ch. III, 8, showing the outline, however, only of the original Bed between the several permanent masonry floors (instead of that of the existing Bed, which seemed unnecessary for this purpose).

The figure assumed by the Free Surface along the Reach at various levels has been drawn upon the longitudinal sections by plotting at each Gauge the MEAN WATER-LEVEL (as defined by the Mean Gauge-Reading h or H) from certain SERIES of velocity-work, viz., from every Series in the Jaolí Reach, and from certain selected Series in the Roorkee Reach. For every such Series, the Gauge-Readings of at least three Gauges, (viz., at the Head, at an Experimental Site, and at the Tail,) are available: and in the case of Series of velocity-work at the 15th Mile and Soláni Embankment Main Sites, a fourth Gauge-Reading (at the Soláni Aqueduct) is also available. Straight lines joining these points show of course (in a rough way) the outlines of the FREE-SURFACE along the Reach.

The Average Height of Obstruction upon Crest of Falls is shown by the thick black lines raised upon the Crest of the Falls to the left of each Plate: the height of these shows in fact the (average) height of the temporarily raised Crest, (or in other words of the Obstruction.)

[To exhibit this distinctly for each Series, these lines—which would in reality overlap—have been plotted separately, slightly spaced out over a small horizontal width, which may be looked on as an enormously enlarged pictorial representation of the width of the masonry Crest: similarly the thick lines in question may be looked on as pictorial representations of the cross-section of the "Sleepers" used to raise the Crest temporarily].

The Free Surface Lines have been carried out beyond the Tail-Gauge in both Plates, and connected with their several (vertical) Obstruction-Lines to enable the eye to trace their connexion. The Serial Nos. attached will also help the eye in tracing

out individual lines on the Plates, and further enable reference to be made to the Abstract Table 1.

16b. *Abstract Table* 1.—To save reference to the details, an Abstract of all the data required is given in Abstract Table 1. This shows the several Serial Nos. and number of Sets in each Series: and the *mean values for each Series* of the following quantities :—

1°. Elements showing Control at Head of Roorkee Reach as in Art 18a.
2°. Elements showing Control at Tail of either Reach (viz., Q, *k* of Art. 18d, e).
3°. Readings of the several Gauges (*i.e.*, from 3 to 4 for each Series).
4°. Resulting Surface-Falls (F_1, F_2, F_3, F) in each Sub-Reach.
5°. Local Surface-Slopes (S), in many cases.

16c. *Roorkee Reach Diagram*, (Pl. VIII).—To prevent confusion, the sloping lines which show the Free Surface for Series of velocity-work done at the various Sites in the Roorkee Reach (Pl. VIII) have been drawn in different styles (*e.g.*, clear, dotted, chain-dotted, and in long dashes, as detailed on the Plate itself, *q. v.*) for each Site. Also each such Line is drawn continuously only across those GAUGES from which it was plotted, (as detailed in the Reference Table, Pl. VIII,) and is *broken where crossing other Gauges.* Many of these lines are also interrupted, to save confusion where much interlacing occurs, the connexion being indicated by arrows, and by repeating the Serial Nos.

16d. *Selection of Series.*—The Results available in the Roorkee Reach are so numerous as to produce simply a confused mass of lines when all plotted together. A selection was accordingly made first of all those containing a complete record of the Obstruction at the Tail of the Reach, (thus rejecting a great many, Art. 18f): among these again preference was given to those containing readings of *four* Gauges, *i.e.*, Series at the 15th Mile and Soláni Embankment Main Site. After taking as many of these as could be conveniently plotted without confusion (*i.e.*, after actual trial), a further selection was made of such Series as contained a nearly perfect record of the Obstruction at Tail, and seemed otherwise instructive.

16e. Free Surface Figure, (Pl. VIII, IX).—First let it be observed that, if the Falls at the Tail were unobstructed, the gradient of the Free Surface might be expected to be *steeper than the Bed-Slope*, especially near the Tail, in consequence of the "draw" of the Falls. But the permanent Obstruction at Tail, (the raised Crest of the Falls,) has had the effect of flattening the gradient of the Free Surface near the Falls to such an extent that at time of Highest Supply the Free Surface throughout the Reach is *roughly parallel to the original Bed-Slope*, (this is best seen in Pl. VIII.)

[The raising of the Crest of the Falls may, therefore, be said to have effected its object, viz., the reducing the gradient of the Free Surface of the Lower Sub-Reach so as never to exceed that of the Upper Sub-Reaches].

The following Conclusions (completely in accordance with the preceding) may be drawn from these Diagrams :—

"In times of High Supply, with therefore Deep Water in the Upper Sub-Reaches, when there is no temporary Obstruction at Tail, the Free Surface sinks as follows:—

in nearly parallel lines in the Upper Sub-Reaches,..............................(30*a*),

in converging lines, (*i. e.*, with gradually flattening gradient, or diminishing Surface-Fall) in the Lower Sub-Reaches",..(30*b*),

[*see* Ser. 191 to 197, 151 to 161, 106, 112 to 125 in support of the above].

"Obstruction at the Tail flattens the Free Surface Gradient generally for a long distance back". [*See* Ser. 116, 120, 127, 131 to 139, 22, 164 to 181], ...(30*c*).

"This flattening takes place with far greatest effect over a certain stretch (which may be called the OBSTRUCTED SUB-REACH), defined by a level line through the Crest of the Obstruction". [*See* Ser. 22, 164, 176, 180, 181], ...(30*d*),

"and with effect rapidly diminishing with increase of distance above the said Obstructed Sub-Reach". [*See* Series 116, 120, 127, 131 to 139, 22, 164 to 181], (30*e*).

"With same depth at any Gauge increased Obstruction causes increased flattening (or, which is the same thing, less Surface-Fall) in the portion of Reach below that Gauge", ...(30*f*).

[*Upper Gauge* (No. 1); Compare Ser. 164, 120; 125, 168; 136, 168; 181, 176; 176, 173; 180, 171; 127, 172.

Central Gauge (No. 4); Compare Ser. 161, 22; 164 with 131, 171, 172; 168 with 131, 171, 172; 176, 178].

"The figure of the Free Surface is a nearly uniform slope at Highest Supply, and is *concave* below Highest Supply, the concavity increasing as the water-level sinks, and increasing rapidly, and extending further back with increase of Obstruction at Tail". [This is of course involved in what precedes], ...(30*g*).

[The existence of *concavity* is clearly due to the Obstruction at Tail of Reach: were the Falls unobstructed, the figure would undoubtedly be convex (if the Bed-slope were uniform)].

Jaoli Reach, (Pl. IX).—The Range of Depth and of Obstruction is small; it suffices to say that the Diagram confirms the above Conclusions.

17. General Conclusions.—The general Conclusions from the above discussions are that—

"The Surface-Gradient at different parts of a Reach is chiefly determined by the state of Control at the Tail", ...(31*a*).

"The power of Control capable of being exercised in the Ganges Canal is so great, that it forms the chief element in regulating the mode of passage of the water (*i. e.*, with high or low surface-gradient) through each Reach",..........(31*b*).

And it will be seen hereafter that Velocity and Discharge at any Site are thus really more dependent on this element than on any other: so that in fact the mere item of "depth of water" or "state of Gauge" at a Site is *no criterion whatever of the Velocity or Discharge* through the Site.

CHAPTER VIII.

SURFACE-CONVEXITY.

Preface.—The subject of this Chapter—Experiments on Surface-Convexity—is of high theoretic, but of little practical, interest.

1. Hydraulic Pressure.—Theory indicates that the pressure in a fluid in motion is always less than the hydrostatic pressure, at any rate in the case of STEADY MOTION, by an amount varying with $wv^2 \div 2g$, (where w is the weight of a cubic unit), and therefore increasing with increase of velocity.

After proving this* for the case of Steady Motion *in a pipe flowing full*, (upon a certain hypothesis as to the law of fluid friction,) the late Canon Moseley quotes† the following Experiment (of Professor Ludwig of Leipsic) in illustration.

Experiment, (Pl. X, *Fig*, 4).—"In the accompanying diagram AB is the section of a pipe filled with water which flows through it. CDE is a continuous glass tube whose straight part CD passes through the pipe in a direction perpendicular to its axis, enters it by stuffing-boxes at *c*, *d*, and is capable of being moved in the direction CD without leakage. *a* and *b* are small apertures in this tube. The pipe being filled with water, the tube also fills with it. But the water in the pipe being in motion, and the aperture *a* nearer to the axis than *b*, the pressure at *a* is by ‡equation (20) less§ than that on *b*. The water from the pipe therefore flows along the tube through *b* in the direction DEC, as shown by the arrow * * * and the air-bubble." [The air-bubble is at *e*].

This would seem to be a positive (experimental) proof that the greater the velocity the more the hydraulic pressure falls short of the hydrostatic pressure, in cases of Steady Motion in pipes at any rate, *throughout a transverse section.*

2. Surface-Convexity.—How far this applies to the Unsteady Motion in Open Channels seems doubtful. However, Canon Moseley goes on to generalize the result.

At p. 353 of same work—

"In all streams there cannot but be a tendency in the water to transfer itself from the sides, where the motion is slower and the pressure greater (as shown by‡ equation 19) to the centre, where the motion is quicker and the pressure consequently less—and also to rise from the bottom to the surface, carrying up with it the mud".

And again, at p. 44 of Vol. XLIV of same work, writing of Open Channels—

"As the pressure is everywhere less where the velocity is greater, it is evident that there will be a tendency in the liquid on the surface to flow from the sides of the channel towards the centre, and that thus the velocity of the surface-water at the

* Moseley's Steady Flow, Philos. Maga., Vol. XLII, p. 352. † *Ibidem.*
‡ The numbered equations refer to the Paper quoted.
§ Because the velocity at *a* is greater than at *b*.

centre will be diminished, and the water heaped up, drowning, as it were, the point of greatest velocity in the section ".

Canon Moseley apparently indicates that the water-surface would not be horizontal along a transverse line in the surface, but *would stand higher* (or "heaped up") *near the centre* where the velocity is great, than near the sides where the velocity is small : so that a cross-section of the surface would *always be convex.*

Now two of the above results are undoubtedly true in Open Channels, *i.e.*, agree with Experiment, viz.—

1°, the constant transference of the surface-water from the sides towards the centre, *see* Ch. XVII, 14b.

2°, the depression, as a rule, of the maximum velocity-line below the surface, *see* Ch. X, 8.

But evidence as to the "heaping up" of the water over lines of quicker motion (*e.g.*, near the centre), or in other words, of the convexity of the surface is almost wholly wanting.

2a. Small Scale Evidence.—In the Atlas of Plates illustrating Bazin's Experiments on Open Channels, there are 46 cross-sections of various small Open Channels (not exceeding 6'½ in width), in which the cross-section of the water-surface is also given in such a way that it is apparently derived from careful measurement of the surface-level at many parts of the section. The following Table shows an Abstract of the number of cases in which the surface is—

1°, higher at the centre than at both sides, (9 cases,)

2°, level across, (10 cases,)

3°, lower at the centre than at both sides, (8 cases,)

4°, other cases not falling under 1°, 2°, 3°, (19 cases,)

separately for each of the four cross-sections figured, (and with complete references to enable the reader to verify the conditions.)

Surface.	Number of Cases. Cross Sections. Rectangle.	Trapezoid.	Triangle.	Semicircle.	Total.	Reference to Bazin's Atlas. Cross-Sections. Rectangle. Plate. Figures.	Trapezoid. Plate. Fig.	Triangle. Plate. Fig.	Semicircle. Plate. Fig.
1°. Centre higher than both sides,	6	0	0	3	9	XIX, 5,9, XXI, 1, XXVI, 5,7,8,	... ...	... ...	XXII, 2,6,8. ... ...
2°. Level across,	6	3	0	1	10	XX, 1,2,5,6,7,8,	XXIII, 1,5,6,	...	XXII, 5.
3°. Centre lower than both sides,	4	3	0	1	8	XIX, 1,2,3, XX, 10,	XXI, 3,4, XXIII, 2,	... ...	XXII, 3. ...
4°. Other cases, [not falling under 1°,2°,3°],	11	4	2	2	19	XIX, 4,6,7,8, XX, 3,4,9,11,12, XXI, 2, XXVI, 6,	XXI, 5,6, XXIII, 3,4, ...	XXI, 7,8, ...	XXII, 1,7. ... ...
Totals, ...	27	10	2	7	46	...	...	...	

So far there is no evidence of the existence of a central elevation, *as a general rule*, there being in all,—

9 cases of central elevation, 8 cases of central depression, and 29 other cases.

The actual amounts of the central elevation and central depression are given (in millimètres) in following Table:—

		CENTRAL ELEVATION (9 Cases).									CENTRAL DEPRESSION (8 Cases)							
Cross Section.		Rectangle.						Semi-circle.			Rectangle.				Trapezoid.			Semi-circle.
Reference [to Bazin's Atlas].	Plate, ...	XIX		XXI	XXVI			XXII			XIX			XX	XXI		XXIII	XXII
	Fig., ...	5	9	1	5	7	8	2	6	8	1	2	3	10	8	4	2	3
ELEVATION OR DEPRESSION [in millimètres]. Above or below water surface at	Left Bank,	2	6	3	3	12	10	28	13	5	11	10	2	4	1	2	5	2
	Right Bank,	6	4	8	1	1	4	26	15	3	17	1	3	4	2	1	6	8

Remembering that 1 millimètre = about $\frac{1}{25}$ inch, it seems probable that—considering the difficulty of the measurement close to the edge—the cases in which the central elevation or depression falls short of 3 millimètres, (about $\frac{1}{8}$ inch,) at either bank should be rejected, and should be classed under Class 4° above.

The number of cases in which the central elevation or depression is a tolerably marked quantity (exceeding say 2 millimètres) *at both banks* is only 6 of elevation, and 8 of depression, out of the whole 46 cases.

Thus the evidence (from these Experiments) is quite trifling.

2b. Large Scale Evidence.—The evidence from Experiments on the large scale is still more meagre. The author has been able to find record of only one such, (and that only at† second hand, *quoted* at pp. 195, 196 of the Mississippi Report.)

"The Annales des Ponts et Chaussées for 1848 contains a long and exceedingly interesting Memoir by M. Baumgarten upon a portion of the Garonne * * * He reports some interesting and unique measurements upon the transverse section of the water-surface at a nearly straight portion of the river (width about 600 feet) both when the water was rising and falling. When rising, at the rate of about 5 feet in twenty-four hours, with a maximum velocity of about 7 feet per second, he found the water in the middle to be about 0·4 of a foot above that on the right bank, and 0·1 above that on the left. When falling, at the rate of about 8 feet in twenty-four hours, with a maximum velocity of about 7·5 feet per second, the water-surface was sensibly a plane, being at the right bank a little less than 0·1 of a foot above its level at the opposite side of the river. The velocities at the banks are unfortunately not given in either case".

3. Measurement of convexity.—On account of the high theoretic

† The author has not been able to obtain a copy of the original from any public Library in Northern India.

interest of this question, it seemed right to make an attempt to test it experimentally in a wide channel.

3a. *Convexity expected.*—It was supposed*—from the theoretical grounds above—that the quantity to be measured, viz., the Elevation of the surface at the centre above the surface at the banks would be of the order of the—

"Difference of the heights due to the velocities at the surface-centre, and surface-margin", *i.e.*, comparable with $(v_o^2 - v_b^2) \div 2g$,

where, v_o = central surface-velocity, v_b = margin surface-velocity.

Now in the Solání Embankment at high water, the central surface velocity (v_o) commonly exceeds 4' per sec., and the margin surface velocity (v_b) is always very small, say less than ·5 foot per sec. With these values, and taking $g = 32$, the rise at the centre might be expected to be—

$$(v_o^2 - v_b^2) \div 2g = \left\{4^2 - (\tfrac{1}{2})^2\right\} \div 64 = (\tfrac{1}{4} - \tfrac{1}{256}) \text{ feet} = 3 \text{ inches nearly,}$$

a quantity so large, that it could not fail to be discovered on actual trial.

But on actual trial, it was found, on the contrary, that the quantity to be measured, the RISE at the centre was certainly a *very small* quantity; (if indeed possessing any real existence.)

3b. DELICACY OF THE OBSERVATION.—The whole water-surface being in a constant state of small but rapid oscillation, and also possibly of gentle oscillation of long period, renders the measurement of *small differences of level* between different parts of its surface extremely difficult. And for reasons similar to those given in Ch. VII, 2a.

"The Water-lével must be taken *simultaneously* at the centre and banks",......(1), and also—

"The Water-levels sought are the means of the "highest maxima" and "lowest minima" recorded within the same short interval (say half a minute)",..........(2).

Again, it is clear that, inasmuch as irregularities of the banks are themselves capable of producing backwaters and eddies, and possible variations of surface-level, the Experiment ought to be tried in a *long uniform straight reach with regular banks.*

[The Solání Embankment is an *unusually favorable* Reach for this purpose, (*see* Ch. III, 9 :) the Experiment was performed at the Main Site, at which nearly all the velocity-work was done].

The determination of the water-level at the centre is a matter of very great difficulty, it being difficult to define the position of the point on the surface (which is to be observed from the banks) without actually touching, and thereby considerably ruffling the water-surface to an extent likely to mask the phenomenon sought. Even with the aid of a Bridge, it would be a matter of great difficulty, but the difficulty is much increased if no Bridge be available.

[The author has not been able to obtain access to any works explaining how other Experimenters have attempted this. None of the Canal Bridges near Roorkee were available for this purpose, as their Piers and towing paths completely destroy the general regularity of the motion of the water].

4. Experiment, (Pl. X, 1, 2, 3).—In the absence of the aid of a Bridge, the mode adopted was as follows :—

* This supposition was accepted in the Mississippi Report (p. 303).

A thin ($\frac{1}{4}''$ girth) wire rope was strained tightly across the Canal at the Solání Embankment Main Site, the straining being effected with the straining screws (shown at S′, S″) used for straining telegraph wires. A small brass "saddle" (*m*) was fitted to slide along the wire, carrying two brass clamps suitable for gripping Levelling Staves. Two ordinary Levelling Staves (graduated to ·01 of a foot) (C′c′, C″c″) 5′ long were carried by these clamps, and being of equal weight balanced each other, one on each side of the Wire Rope; one staff facing full to either bank. Two thin strips of sheet brass (c′, c″), 6″ long by $1\frac{1}{2}''$ wide by $\frac{1}{32}''$ thick, graduated on one side precisely like a Levelling Staff, were screwed on to the feet of the staves, in such a way that their graduated faces appeared like *continuations of the graduated faces* of the Staves (*Fig.* 3), and continued downwards below their zeros. When in position for use, the Staves were clamped in such a position that the two thin brass Strips dipped about 3″ into the water, and were turned with their thin edges to the current.

The arrangements for taking *at the same time* the "highest maximum" and "lowest minimum" water-levels at the centre c′, c″, and at both banks, B′, B″ will now be understood from the Diagrams, (Pl. X, 1, 2.)

S′S″ is the Wire-Rope. C′c′, C″c″ is the pair of Levelling Staves balanced on either side of the Wire at mid-channel *m*. c′, c″ are the thin brass strips dipping into the water. B′B″ is the water-surface.

T′, T″ are two theodolites placed on the banks opposite to the Staves C′, C″, with their telescopes pointed down to the water-surface at c′, c″ respectively.

L′, L″ are two Levels in good adjustment at 100′ down-stream, carefully levelled and pointed towards the Staves C′, C″ respectively.

B′, B″ are two of the thin Sheet Brass Foot-Rules described in Ch. V, 7a, placed on the top of the highest wetted step on each bank, with their thin edges to the current, wherewith to measure the height of the water-surface above the step in question.

Field-work. Six Observers worked* in concert, viz., one at each Theodolite telescope T′, T″; one at each Level L′, L″; and one at each Foot-Rule B′, B″.

All four telescopes T′, T″, L′, L″ having been carefully focussed, the six Observers applied themselves at a given signal to their several Instruments (T′, T″, L′, L″, B′, B″), and obtained first the *approximate* readings. After which two more signals were given at about one half minute interval, within which space of time each Observer recorded the "highest maximum", and "lowest minimum" reading.

This operation was repeated many times in succession. Lastly, Levelling Staves being placed on the top of the Bench-marks at B′, B″, both Levels were directed in succession *on both Staves* and the readings taken.

All the data requisite are now obtained.

Let H′ = reading of Staff upon Step B′, as read from Level L′,
" h′ = reading of brass Foot-Rule at B′,
" C′ = reading of Staff C′ as read from Level L′,
" c′ = reading on strip at foot of Staff C′ as seen from theodolite T′,
} the single accents referring to the Left Bank,

and let the same symbols *with double accents* refer to the Right Bank.

* The author's acknowledgments are due to the Staff and Students of the Thomason C. E. College who assisted in this difficult Experiment, viz., to Messrs. C. C. Sullivan, J. H. Fairley, J. Low, and W. Hay, Head and Asst. Masters of the Upper Subordinate Class; and to Mr. J. T. Farrant, Mr. W. A. B. Swinnerton, and Bábú Battú Lál, Students of the Engr. Class.

Then it is clear that, (*see* Pl. XX, 11)—

$H' - h'$ = depression of water-surface at Bank B' } below the level plane { .....(3*a*),
$C' + c'$ = depression of water-surface at centre C' } defined by the Level L' { .....(3*b*),

$\therefore (H'-h')-(C'+c')$ = Elevation of water-surface at centre c' above water-surface at left bank B',..(4*a*),

and $(H''-h'')-(C''+c'')$ = Elevation of water-surface at centre c'' above water-surface at right bank B'',..(4*b*),

it being understood that h', C', c'; h'', C'', c'') are in each case the arithmetic mean of the "highest maximum", and "lowest minimum", *observed within the same half minute.*

The Results above obtained are affected by the personal equations of the Observers, and by any imperfections in the Instruments L', L''. To eliminate any error hence arising, after a given number of operations, the Observers doing similar work on opposite banks changed places (*i.e.*, T' with T'', L' with L'', B' with B'') *carrying their Instruments with them;* and an equal number of observations were made in their new positions, ending as before with reading Staves placed on both Bench-marks B', B'' from both Levels.

From the symmetry of the geometric figures on either side about the axis of the stream (*see* Plan), and from the mode of interchange of Observers and Instruments, it is believed that the effects of "personal equation" and of instrumental errors must be eliminated from the mean of the whole.

4a. TRIAL.—The Experiment was tried on two different days, viz.,
12 times on each bank on 19th May 1877, } in the early morning
24 times on each bank on 23rd June 1877, } on both occasions.

[Every possible care was taken to make the whole of the Observations trustworthy. Both Levels were excellent 20" Instruments: their adjustments were tested just before each Experiment, and an additional Assistant watched the correctness of position of the bubble of the spirit-level *during the whole time that readings were being made*].

The Experiment was a very tedious and difficult one. The rush of water past the feet of the central Staves carried the Staves slightly out of the perpendicular, and the water stood a little higher at the up-stream edge than at the down-stream edge of their feet: the water-level was always taken at the down-stream edge.

Again, small weeds repeatedly caught in the feet of the central Staves, and caused so much disturbance of the water as to render further reading useless until the weeds were freed. Every time this occurred a boat had to be sent out to free the weeds: the disentangling of the weeds gave a jerk to the wire-rope, setting it oscillating violently: no further readings could be taken till the oscillation from this cause subsided.

[In consequence again of the Experiments being done in the early morning, the Staff C' facing the East (or left) bank was in much better light than the Staff C'' facing the West (or right bank); the foot c'' of the Staff C'' being in shadow, it was difficult to see clearly where the water cut it. An attempt was made to light up the foot c'' of the Staff C'' with a heliostat, but the sky was cloudy on both occasions, and the attempt failed].

The details of the Data and Results are given in Tab. LXXVII, LXXVIII: an Abstract of the Results is given in Table below.

Date, 1877.	Central depth.	Wind.	State of Canal.	Number of trials.	Maximum Oscillations.			Elevation of Water-surface at Centre above						Approx. Surface-velocity.	
					Left Bank.	Centre.	Right Bank.	Left Bank.			Right Bank.			Centre.	6' from edge.
								from	to	Mean.	from	to	Mean.		
19-5	10'·4	Calm	Rising	12	?	?	?	–·016	+·009	**–·003**	–·044	+·018	**–·014**	4·50	1·50
23-6	11'·2	Calm	Rising	24	·060	·070	·040	–·095	+·016	**–·009**	–·041	+·014	**–·007**	4·49	1·40

It will be seen that the Results are very variable and contradictory; some giving elevations (shown by the + sign) some depression (shown by the – sign) of the surface at the centre above that at the banks; but agreeing in showing a (small) DEPRESSION as the *average result* on both days.

[The detailed Results cannot of course pretend to accuracy in the third place of decimals: but the MEANS (black letter figures) are probably very approximate Average values. The magnitude of the Oscillations of the water-level is of course the great difficulty.

The surface-velocities shown were *not taken at the time*, but are average values such as would usually correspond to the depth].

5. Surface-Convexity doubtful.—The remarkable Result of the two Experiments just detailed, done with every possible care, viz., a small DEPRESSION at the centre, as the *average* Result of both day's work, cannot fail to throw doubt on the correctness of the Theory which leads to the expectation of convexity of the surface. The Depression is perhaps too small—considering the difficulty of the Experiment—to admit of the expectation of surface-concavity as a general Rule: but the absence of convexity *on the average* is at any rate clear. It may be fairly concluded that—

"The surface of water in motion in a long straight Reach with pretty uniform Banks is—on the average—nearly level across".....................................(5).

END OF PART I.

PART II.

CHAPTER IX.

SUBSURFACE VELOCITY INSTRUMENTS.

Preface.—Full details of the construction of, use of, and objections to, the Double-Float are given in this Chapter. Those who do not require such full detail should read only Art. 2, 2a, 4, 7, 9, 9a, & 12.

1. **Subsurface-Velocity.**—The measurement of the velocity at any point beneath the surface is the first step towards obtaining an experimental knowledge of the subsurface motion. The measurement is unfortunately attended with great practical difficulties. Many different Instruments, of which a brief description will be found in Weisbach's Mechanics of Engineering, Vol. I, Art. 378, *et seq.*, and in the Mississippi Report, page 202, *et seq.*, have been proposed for this purpose, but none of them can be said to be quite satisfactory, being all open to various serious objections.

The only ones which have met with extended use are the three following:—

i. Double-Float, used in the Mississippi, Lake River, Connecticut, Irrawaddi, and Roorkee Experiments.

ii. Current-Meter, used in the Lake River, Rhine, Elbe, La Plata, and Connecticut Experiments.

iii. Pitot's Tube, used in the Darcy-Bazin Experiments, (small scale.)

The description of, and discussion about the use of, the Double-Float will occupy the rest of this Chapter.

2. **Double-Float.**—This Instrument is made of many various patterns and sizes, but consists essentially of only *three* parts, viz., a *heavy* Sub-Float attached to a *light* Surface-Float, by a *thin* cord or wire, which will for shortness be called the Connector.

On being abandoned to the current, the heavy Sub-Float gradually sinks until it draws the Connector taut, and by its means receives the requisite support from the Surface-Float, after which the motion of the Instrument is for a time irregular: after a time it attains a state of

relative equilibrium, and finally moves with a "terminal velocity", which is the resultant action of the current on its several parts. This (terminal) velocity is the velocity to be observed.

The deduction of the velocity at a point beneath the surface from the (observed) velocity of such an Instrument, requires in strictness that the relative current-actions on its several parts should be separately known, a Problem more difficult than the one in hand. An approximate deduction of the subsurface velocity may, however, be made with this Instrument by so proportioning the size, shape, and physical state of its several parts, that the current-action on the Connector may be relatively negligible (compared with the action on the Sub-Float), and that the current-action on the Surface-Float may be either—

1°, relatively negligible compared with the action on the Sub-Float.

2°, related to the action on the Sub-Float in a known manner, so as to admit of elimination (by calculation).

2a. ORDINARY, and TWIN PATTERNS.—This last consideration gives rise to two essentially different patterns of Double-Float, differing radically only in the relative size of the Surface-Float, viz.—

1°. ORDINARY (with small surface-float). In this—which is the ordinary type—the Surface-Float is made of the smallest size compatible with the various practical requisites, so that the current-action on it may be, if possible, relatively negligible, (compared with the action on the Sub-Float.)

2°. TWIN-FLOATS. In this form the Sub-Float and submerged parts of the Surface-Float are made of the same size, shape, and condition of surface. This renders possible the calculation* and elimination of the current-action on the Surface-Float.

2b. TWIN-FLOATS REJECTED.—The first pattern (with small Surface-Float) is the only one which has been extensively used. As, however, the Double-Float was the Instrument selected (for subsurface velocity-measurement) in the present Experiments, it seemed of great importance to give both patterns a fair trial. Some pretty extensive comparative Experiments were, therefore, undertaken at an early period of the work (March 1875) *with every possible precaution to make the Test a fair yet crucial one.*

Experiment. The Instruments to be tested (for detailed description, *see* Art. 40, 48 of 1874-75 Report) were prepared so as to be as nearly as possible *alike in all respects*, except the essential points of dissimilarity, viz., size of Surface-Float, and weight of Sub-Float. The Experiments consisted of repeated velocity-measurements *done one by one with each Instrument in turn in rapid succession*, (so as to secure close similarity of the "External Conditions",) viz.—

156 trials of each at 6′ depth, in water from 6′·6 to 7′·55 deep,

* It is shown in Art. 49 of the 1874-75 Report, that upon a certain Theory of current-pressure the Subsurface Velocity (v) is given by the simple expression $v = 2u - v_0$, where u = velocity of Instrument, v_0 = surface-velocity.

60 trials of each at every foot of depth from 1' to 9' deep (or 540 trials in all), in water from 9'·25 to 9'·50 deep,

all the trials being at mid-channel, (in which line accidental deviations to right or left affect the result least.) Full details of these trials will be found in Art. 55, 56, 65 of the 1874-75 Report.

The general conclusions arrived at were, *in respect to the patterns tried*—

i. *Ordinary*. "The surface-action on surface-float is by no means inappreciable", ...(1).

ii. *Twin-Floats*. "The surface-action is over-estimated, and the elimination errs in excess", ...(2).

As far as regards effect of surface-action then, the two Instruments are probably on a par. But the "unsteady motion" of the water introduces special difficulties detailed below in the use of the Twin-Floats.

Objections to Twin-Floats. The elimination of the surface-action requires—*see* Art. 49 of 1874-75 Report—a knowledge of the surface-velocity *in the very path traversed by the upper of the Twin-Floats.* Were the motion of the water "steady", it would suffice to measure the surface-velocity a little before or after the passage of the "Twin-Floats". But, in consequence of the great variation (Ch. VI, 4) of the motion of the water, it is further necessary that this surface velocity-measurement should be effected *at the same phase* of its variation as that in which the observation of velocity of the "Twin Balls" is made. To secure strict coincidence either of path or of phase is of course impossible ; but real approximation, especially to the latter (on account of the *rapid variation* of the motion) seems essential to any useful result. An attempt was made to secure this by throwing the "Twin-Floats" and the Surface-Float from the upper boat at such an interval as to reach the Upper Rope nearly together, and therefore pass through the "Run" nearly at same time. The motion of the water varies, however, so rapidly, that it seems certain that the requisite approximation to similarity of phase was not, and in practice cannot be, secured, from which it follows that—

"The result deduced from a single observation with this Instrument is not a fair approximation to the actual subsurface velocity along the path of the Sub-Float at the actual time of its passage", ...(3).

The average of a great many results would of course give the requisite approximation to similarity of phase, because it would in effect be derived from the Average Velocities of both Instruments, (Twin-Floats and Surface-Float,) which are of course comparable, (Ch. VI, 4a.)

Again, in Experiments near the banks (where the surface-motion is irregular), it would seldom happen that both of the Instruments, even when thrown as above described,—*i.e.*, so as to reach the upper rope nearly together—would traverse the "Run" without undue deviation of one or other of them from the proper line, which vitiated that trial. This led to quite undue waste of time.

On account of these difficulties in the use of the Twin-Floats due to the unsteady motion of the water, coupled with the fact that the elimination of the surface-action depends upon a by no means certain Theory, (*see* Art. 49 of 1874-5 Report,) it was decided not to use this Instrument further. Thus the "ordinary" pattern, *i.e.*, with small surface-float was adopted for the present Experiments, and was *exclusively used for subsurface velocity-measurement.*

3. Double-Float, HISTORY*.—The earliest known suggestion of the DOUBLE-FLOAT is by †Leonardo da Vinci (previous to 1643) in form of a slender stick with a mass of stones (as Sub-Float) at one end, and a light mass as (Surface-Float) at the other: but it does not appear whether he applied it to practical velocity-measurement. The earliest record of the practical use of the Double-Float in its modern form is by ‡Marriotte (previous to 1684), in form of two balls of wax connected by a thread, one of which was loaded with small stones to make it sink.

The only material improvement since his time has been the enlarging the size of the Sub-Float, and reducing that of the Surface-Float as much as possible. Since Marriotte's time to that of Dubuat (1779), the "Double-Float" was one of the principal Instruments by which a rough idea of the variation of velocity from surface to bed on the same vertical was obtained.

It has in this way *contributed perhaps more than any other Instrument* to the disproof of the notion (generally accepted in 1750) that the velocity at different points on the same vertical in an open channel followed the law of efflux from small holes in the side of a tall vessel kept constantly full, (known as Torricelli's Theorem: its symbolic expression is $v = \sqrt{2gh}$). For fully a century after Marriotte's time this notion (founded on a supposed but false analogy) proved the most complete hindrance to the advance of the science of Hydraulics; the Double-Float has certainly done one good service in disproving this notion.

In modern times the Double-Float appears to have been almost given up in Europe, but it has been largely and successfully employed in America, viz., in the Mississippi (1851—'58) and Connecticut (1874) Experiments, and also in India, viz., in the Irrawaddi (1872—'79) and Roorkee Experiments (1874—'79).

4. Double-Float, Essentials of.—The following are the *Special Conditions* to be fulfilled in a good DOUBLE-FLOAT of the ordinary kind *i.e.*, with small surface-float), in addition to the "General Conditions" (Ch. IV, 6) common to every FLOAT, several of which are here repeated in the special forms which they take with this Instrument.

1°. As to the SURFACE-FLOAT—

(*a*), The part exposed to wind should be the least possible consistent with the function of serving as a "marker".

(*b*), The submerged part should be the least possible, so that the surface current-action on it may be *relatively negligible*,

(*c*), and yet it should have excess of buoyancy (above that required for the mere support of the Connector and Sub-Float) sufficient to bring it quickly to the surface after any accidental submergence, so that it may properly serve its function as a marker.

2°. As to the CONNECTOR—

(*a*), It should be the thinnest possible, so that the current-action on it may be *relatively negligible*,

* The matter of this Article has been kindly furnished to the author by Mr. R. Gordon, the Superintendent of the Irrawaddi Experiments.

† "Del Moto e Misura dell'acqua", B. II, 42, in "Raccolta d'Autori Italiani, &c.," Bologna, 1826.

‡ Marriotte, Du Mouvement des Eaux, p. 277.

(*b*), but should be strong enough to bear the weight of the Sub-Float in air, together with an occasional jerk.

3°. As to the SUB-FLOAT—

(*a*), It should be *relatively of large size*, (*i.e.*, compared with the other parts, so that the current-action on it may greatly exceed the actions on those parts,)

(*b*), but should not exceed a certain small size such that the velocity of the current is sensibly constant throughout its extent, the smaller the better. [The maximum admitted in these Experiments was 3" × 3"].

(*c*), It should be of sufficient mean specific gravity to sink rapidly to the full length of the Connector,

(*d*), and also to maintain itself at a nearly constant depth, in spite of upward eddies or currents.

(*e*), It should be so ballasted (if not of spherical shape) as to possess stability sufficient to prevent its being tilted by the pull of the Connector, so much as to sensibly reduce the Area exposed to the Current.

Of the above "special conditions", Nos. 1°*a*, *b* (of the Surface-Float), No. 2°*a* (of the Connector), and Nos. 3°*a*, *b*, *d*, *e* (of the Sub-Float) are essential to the requisite delicacy of the Instrument, *especially* Nos. 1°*b*, 2°*a*, 3°*a*, *d*, *e* which are simply *all important*. Nos. 1°*c* (of the Surface-Float), 2°*b* (of the Connector), and 3°*c* (of the Sub-Float) are practical conditions essential to the convenient working of the Instrument, and are, therefore, nearly as important as the preceding.

The simultaneous fulfilment of all these Conditions is a matter of great difficulty, especially when an attempt is made to fulfil No. 3°*b* of making the Sub-Float the smallest possible. In fact it is clear that several of the Conditions are *inconsistent*.

[Thus Nos. 1°*a*, *b* are inconsistent with 1°*c*, and also with 3°*c*, *d*; Nos. 3°*a*, *b* are also mutually inconsistent, so that their "ensemble" can only be partially fulfilled by a compromise, and the best Instrument will be that in which this compromise is most skilfully made].

5. *Precautions in use.*—When the Double-Float is thrown into the current, the Surface-Float and Sub-Float move at first independently: but the Sub-Float gradually sinks until it draws the Connector taut, thereby giving the Surface-Float a considerable jerk which is (or may be) *visible from the shore*: after this the Sub-Float and Connector gradually modify the motion of the Surface-Float, thereby communicating to it a series of slight jerks—which are (or may be) visible from the bank—which gradually die away: finally a state of relative equilibrium is attained, on which the jerking motion ceases. After this the Instrument is "in proper train" for observation. All the stages of irregular motion should of course take place before the Instrument reaches the Upper Rope, so that it may enter the Run "in proper train."

The attainment of this stage of course takes time, and longer and longer as the

length of Connector is increased, in consequence of the time required for the Sub-Float to sink to its final depth. Hence the Dead Run (or distance from Upper Boat to Upper Rope) must be gradually increased as the depth to which the Sub-Float has to sink is increased.

[In the present Experiments the jerking motion of the Surface-Float above mentioned was generally visible from the shore. The Observers had instructions not to record the passage of any Double-Float not known to be "in proper train" as evidenced by the setting in and final cessation of the jerking.

Even after the jerking has ceased, the continuance of control of the Sub-Float over the Surface-Float causes a slight ruffling of the water round the Surface-Float (which does not exist with a detached Surface-Float): this was often visible from the shore on calm days when the water-surface was smooth. As to the actual length of Dead Run used, *see* Ch. IV, 23].

6. Adverse Opinions.—It will be clear from what precedes that no Double-Float of the ordinary kind (*i.e.*, with small Surface-Float) can be at all a perfect Instrument, but must give subsurface velocity-measurements affected by the current-actions on both the Surface-Float and Connector. This would be of little practical importance if the approximation were sufficient.

This is a point on which there is unfortunately much disagreement among hydraulicians. Some go so far as to say that the Instrument is utterly untrustworthy. As this Instrument was the one adopted in these Experiments for all subsurface velocity-measurement, it seems right to state fairly the objections that have been made, and to endeavor to meet them.

One of the most vigorous condemnations of the Double-Float is that of Mr. Révy, who after detailing his objections to it—comments thus (p. 8 of Révy Report) upon the Mississippi Survey—

"The engineers of that survey relied entirely on floats, and we consider it a misfortune to science and to practical engineering that so much ability, perseverance, and time should have been spent to obtain results which the unfortunate choice of floats has inconveniently marred and confused".

Again, in respect to the Roorkee Experiments of 1874-5 (pub. in 1874-5 Report), Mr. Révy thus expresses himself in a criticism communicated to the author on 15th July 1876—

"Mr. Révy believes Capt. Cunningham's observations of considerable negative value. His experiments show—with still greater force than those on the Mississippi—that floats are a hopeless contrivance to disentangle the laws which govern the movement of water in confined or in open channels".

Both Double-Floats and Current-Meters were used on the American Lake River Survey. The opinion of the Experimenter was wholly adverse to the Double-Float, (Lake River Report of 1869, p. 563,) but this opinion was not upheld by the Chief Engineer reviewing the work, (same Work, pp. 620—628).

The weight of the opinions adverse to the Double-Float is considerably reduced by the fact that some of the principal objectors (*e.g.*, Mr. Bazin and Révy) have not themselves had much experience with it.

Thus Mr. Révy's objections (pp. 4 to 8 of Révy Expts.) are based solely on speculative grounds (*i.e.*, not on actual experience), and are prefaced with the statement (*ib.*, p. 4)—

"We had a natural aversion to floats as a means to determine the current of a river. It appeared to us a ready-rough way to observe currents".

Again, Mr. Bazin's published objections (pp. 328—348 of "Discussion, &c.,") are based on the disagreement of certain results of the Mississippi and Irrawaddi Experiments (done with the Double-Float) from the results of like kind of the European Experiments (done with Current-Meters and Pitot's Tube). Mr. Bazin seems inclined to attribute all the disagreements to the faults of the Double-Float, without apparently making any allowance for the chance that some, at any rate, of the divergences in question are perhaps those *really existing* between mighty rivers with small surface-slope (like the Mississippi and Irrawaddi) and small rivers and small canals with large surface-slope (like those of the European Experiments).

However, Mr. Bazin has withdrawn some of these objections in a communication to the author of 21st April 1877, in following terms:—

"I do not condemn the Double-Floats so absolutely as you seem to think. Under conditions of moderate depth and velocity, they certainly work well: but with large velocities and great depths we cannot really tell (at least I am led to think so) what becomes of these Floats. The Experiments on the Connecticut River, in which the Double-Floats have worked well, are not in accord with those of the Mississippi. However this may be, there is herein, it seems to me, an obscurity to be cleared up".

7. Opinions in favor.—On the other hand some of those who have had large experience with the Double-Float have *selected it after trial of other Instruments.*

Mississippi Report, (p. 224.) About subsurface velocity-measurement, it is stated—

"Of all the methods known for determining this quantity, that by double floats was found to give the best results. A few measurements of the velocity of tributary streams, where both banks were submerged, were made with a ship's log; and some few observations were taken at the mouth of the river with Saxton's current-meter; but for all other velocity observations, the double float was exclusively used".

And again (on p. 225)—"Only double floats were found to give reliable results".

Connecticut Report. Both Double-Floats and Current-Meters (of two patterns) were *extensively used* (partly in consequence of the controversy about the Double-Float), so that the opinions published are of unusual weight. It is stated (Report (of 1875, p. 306; or of 1878, p. 307)—

"In the gauging of the Connecticut at Thompsonville, both floats and meters have been used with satisfactory results".

And again (p. 305, or p. 306 of Works quoted)—

"Double-floats, all things considered, have proved to be the most reliable means of measuring subsurface velocities where a sufficiently uniform channel can be found in which to use them. The apparatus is cheap and simple, and the results apparent,

admitting of no unperceived large error. It can be used at all velocities, measuring accurately even the smallest currents; and the best experiments and data known in river hydraulics have been made with it".

Can there be higher praise than this, coming from Experimenters who have extensively used both Instruments?

[It was this opinion, published in 1875, which seemed to the author sufficiently weighty to warrant the exclusive dependence on the Sub-Float in the present Experiments].

Again, Mr. Robt. Gordon (Supdt. of the Irrawaddi Experiments) writes in a note sent to the author on 18th April 1878—

"My own experience, which has been very great on the largest scale, is in favor of the Double-Float".

8. Detailed Objections.—The only—and it must be admitted that some are very serious—objections to the use of the Double-Float *on the score of inaccuracy* seem to be the eight following:—

i. Lateral Deviation of Sub-Float.
ii. Surface-Float Resistance.
iii. Connector-Resistance.
iv. Lift of Sub-Float due to its lateral deviation.
v. Lift of Sub-Float due to its "lag" or "lead".
vi. Lift of Sub-Float due to curvature of Connector.
vii. Instability of Sub-Float.
viii. Tilt of the Sub-Float (when non-spherical).

It will be well to examine these in detail.

[The Articles in which these Faults are discussed bear—for ready reference—the same subordinate numbering (i—viii) as in the above numeration].

8, i. *Deviation of Sub-Float.*—Lateral Currents are apt to cause a *lateral separation* between the paths of the Surface and Sub-Float. The Sub-Float being usually out of sight, the question of the Instrument passing through the Run in "fair course", and also the position (distance to right or left of the current-axis) of the Float-path of the Sub-Float, which is what is really required, can only be judged from the (visible) Surface-Float, thus introducing an element of uncertainty into both questions.

This uncertainty is of little importance so long as the lateral separation of the Surface and Sub-Float does not carry them into planes in which the velocity at the same level is sensibly different: thus it is of little importance at and near mid-channel, but becomes of more importance with approach to the banks, and is of most importance close to the banks, inasmuch as near the banks a small Deviation of the Sub-Float from the Float-Course indicated by the Surface-Float carries it into stream-lines of sensibly different velocity.

Nothing is known for certain as to whether this effect increases with the depth or not.

8, ii. *Surface-Float Resistance.*—The surface-action on the Surface-Float *accelerates or retards* the Sub-Float according as the surface-velocity is *greater or less* than the velocity of the stratum in which the Sub-Float moves. This effect is usually greatest when the Sub-Float is near the bed where the velocity is least.

8, i, & ii. *Efficiency of Sub-Float.*—When the Surface-Float exposes a much smaller area to the current than the Sub-Float, both effects, just described, viz.,

i, Lateral Separation, and ii, Acceleration or Retardation,

are comparatively small. The experimental evidence that *in a well designed* Instrument the Sub-Float does really efficiently control the Surface-Float is really pretty considerable.

1°. *Mississippi Report* (p. 224). As to the Sub-Float it is stated—

"its size was so much greater than that of the Surface float, that the latter did not sensibly affect the rate of movement. This assumption was tested by placing the apparatus in still water during a high wind, and also by noticing the direction of the paths of the floats during a gale blowing directly across the river. No wind effect of consequence could be detected in either case".

2°. *Present Experts.* The Double-Floats were tried several times *close to the vertical wall* of the central Pier of the Solâni Aqueduct (Pl. II, 4), in clear states of the water, in which the Sub-Float could be seen down to a depth of about 6′ or 7′. The "lateral separation" was never seen to exceed about 2″ when the Instrument was "in train", (Art. 5,) an unimportant quantity.

Again, in the Experiments on the vertical close to that Pier, (only 7″½ off the Pier, *see* Ser. 29, 30, Pl. XVI,) it was found that there was a strongly marked set of the surface water away from the bank, so strong that—even with the use of a Run of only 12′½—as many as 100 surface-floats were sometimes thrown before the standard number of three running in "fair course" (nearly parallel to the bank) was secured. With the Double-Floats on the other hand, there was no such frequent "deviation" from running in "fair course" at depths greater than 1′. The proportion that failed to run "fair" out of the whole number thrown was not unusually large in spite of the surface-current tending to carry the surface-float away from the bank.

3°. *Ocean Circulation Evidence* (*see* Art. 63 of Dr. W. B. Carpenter's "Inquiries* on Oceanic Circulation"). The existence of an under-current in the Bosphorus was proved by experiment with a "Current Drag", which consisted of a pair of vanes at right angles, so loaded and tied to the Connector as to hang vertically. In an Experiment on 21st August 1872 with a surface-current outwards of 3½ knots per hour, and N.E. Wind of force 4, the motion of the Drag was found to be *contrary to the surface-current.* It is stated—

"When the current-drag was lowered to a depth, afterwards assumed to be 20 fathoms, it at once rushed violently away against the surface-stream, the large buoy and a small one being pulled completely under water, the third alone remaining visible. It was a wonderful sight to see this series of floats tearing through the water to windward. The steam-cutter had to go full speed to keep pace with it".

This last evidence is particularly valuable, being from an Experiment unconnected with Canal or River Hydraulics. It may be assumed, therefore, that—

"The surface-action on a well designed Double-Float is relatively small",...(4).

This is admitted also by Mr. Bazin (p. 328 of "Discussion").

* pub. in Procgs. of Royal Geographical Society of 1st June 1874

8, iii. *Connector Resistance.*—The Connector is always made as thin as possible, so that the current-action on it may be reduced to a minimum. But, however thin it be made, its area exposed to both direct and lateral current-action increases as its length, so that the efficiency of the Instrument *decreases* on this score alone *with the depth of immersion* of the Sub-Float.

This fault—insensible at small depths—becomes most serious at great depths: it is in fact by far the most serious fault of the Instrument, and yet curiously enough was one of the last to attract attention.

[It seems to have been but little noticed until after the Mississippi Experiments, when it was pointed out by Mr. Bazin (Discussion, p. 328) that in the Double-Float used in those Experiments in which the Connector is stated to have been of $\frac{3}{16}$-inch thickness (Miss. Report, p. 224), the Areas of Connector and Sub-Float directly exposed to current-action were—when in use at the maximum depth of 100′—in the ratio

$$\text{Area of Connector} = 1\tfrac{1}{2} \times \text{Area of Sub-Float},$$

so that the actual velocity of the Instrument could have been no proper approximation to the velocity of the current at the level of the Sub-Float.

This objection has been met* *as far as the Mississippi Experiments of* 1851-53 *are concerned* (but not the rest) by the statement that the thickness $\frac{3}{16}$-inch given in the original Report is a misprint for $\frac{1}{16}$-inch].

8, iv, v, vi. *Lift of Sub-Float.*—The three causes already discussed combine to *lift* the Sub-Float to an unknown (and therefore prejudicial) extent, viz.,

iv. *Lateral Deviation of Sub-Float.* The Sub-Float moves to right or left of the path of the Surface-Float in consequence of lateral currents.

v. *Lag or Lead of Sub-Float.* The Sub-Float lags behind or leads ahead of the Surface-Float according as the velocity of the fluid stratum in which it moves is less or greater than the surface-velocity.

vi. *Connector-Resistance.* The varying current-pressure on different parts of the Connector throws it into a curved or even sinuous form.

Each of these causes separately tend to make the Sub-Float move at a depth (s') less than the full length (s) of the Connector: and, the real depth (s') being unknown, the velocity-measurement made is necessarily *attributed to the depth* (s) *indicated* by the (known) length of the Connector. The amount of "Lift" due to Nos. iv and v is believed to be small; nothing is known as to where that due to No. iv is greatest: that due to No. v is greatest near the bed, where the "lag" of the Sub-Float is greatest: that due to No. vi obviously increases with the length of Connector, and therefore with the depth.

[In the Experiments mentioned under head of "Efficiency of Sub-Float" above, the "Lag" or "Lead" of the Sub-Float never exceeded about 4″ at the greatest visible depth, (about 6′,) a quantity of little importance in shortening a vertical of 6′ depth, and was less at lesser depths. There is, however, direct evidence of the "Lift" amounting occasionally to as much as 6″ in Ser. 14, Tab. XIII, *q. v.*, wherein a Double-Float of the 1½″ Shell pattern with 7′ Connector was run in 6′·63 of water, so that the centre of the Sub-Float could not have been more than 6′·49 immersed, (and might have been less immersed, if any silt had been present,)

* *see* Notes on this point by Genl. Humphreys and Col. Forshey, pub. at p. 372 of the Connecticut Report of 1875.

thus showing a "nominal depth" of 7′ with a real depth not > 6′·49, or in other words, a Lift of about 6″].

8, vii. *Instability of Sub-Float.*—The necessity of using the smallest possible Surface-Float involves using a Connector and Sub-Float whose "effective weight" in water shall be small, and whose sinking power in water is therefore small. The STABILITY of the Sub-Float, *i.e.*, its power of retaining itself at constant depth in spite of occasional upward currents and variations in the upward pull of the Connector is therefore small, so that the Sub-Float is liable to move at a variable depth. This is a serious fault, because an accidental upward impulse given to the Sub-Float by the irregular motion of the water would suddenly reduce the Tension of the Connector, and thereby reduce the control of the Sub-Float upon the motion of the Surface-Float. This is most likely to happen near the bed, where the upward eddies are probably generated.

8, viii. *Tilting of Sub-Float* (if non-spherical).—A non-spherical Sub-Float is liable to be *tilted* by the pull of the Connector (if not applied at its centre of gravity), or by irregular currents out of its normal orientation : the area thereby effectively exposed to current-action is liable to be so greatly reduced, that the relative current-actions on the Surface-Float and Connector are no longer negligible.

This fault of course does not exist in spherical Sub-Floats, but with non-spherical Sub-Floats—especially those of annular or of cross (X) shape—it may be a very serious one ; it is partly controllable by attaching the Connector near the centre of gravity of the Sub-Float, and by ballasting or loading its lower end. This fault is also liable to increase with the depth, because the Total Current-Pressure on the Connector, to which its tilting action is chiefly due, increases with the depth.

[As the Sub-Floats used in the present Experiments were all spherical, this fault need not be further alluded to].

9. Summary of Objections.—It will be seen that most of the faults detailed are of greatest importance near the bed, and that those due to the Connector (Nos. iii and vi) increase steadily with the length of the Connector, and therefore with increase of depth of immersion. In fact these two, and No. vii, are by far the most serious faults of the Instrument.

The first seven faults are inherent in the use of the Instrument, and cannot be entirely removed, but they may be reduced to tolerably small amounts (which is all that is practically required) by properly proportioning the size and weight of the several parts of the Instrument. It is easy to see that increase of size and nett weight (in water) of the Sub-Float will reduce all the most serious faults (Nos. iii, vi, vii), and also No. viii together.

[This of course involves increase of the Surface-Float : but this need not involve increase in Faults Nos. ii and v if the increase of size of the Sub-Float be suitable. Some advantage may also be got by making the Connector of very light material, so that it may itself require but little support from the Surface-Float. This shows that silk thread, cord, &c., are better than wire for the Connector].

From the above follow the important Conclusions —

"The efficiency of a given Double-Float decreases with increase of depth of immersion of Sub-Float",..(5).

"For a given Double-Float there is a limit of depth beyond which it ceases to give a proper approximation to the Subsurface Velocity",...................................(6).

and, lastly,

"To secure equal efficiency at all depths, the Sub-Float should be increased both in size and nett weight as the depth increases", ... (7).

9a. Resultant Error.—Summing up then the effect of all these sources of Error, it is seen that the mode of use of the Double-Float is such that—

"The velocity-measurement (v'), *i. e.*, the observed velocity (v') of the Instrument itself, with the Sub-Float sunk only to depth z', is attributed to the (greater) 'nominal depth' (z) indicated by the length (z) of the Connector (z always $> z'$), and is more or less affected by Surface-Float Action, and Connector Action",...(8).

For this reason in what follows, the depths (z) indicated by the length (z) of the Connector, will be styled Nominal Depth when necessary to distinguish them from real depths.

10. Connector-Length.—The Length (z) of the Connector which indicates the (Nominal) Depth (z) of the velocity-measurement made, is measured in this Work from the under surface of the Surface-Float to the centre of the Sub-Float, (*see* Pl. XI.)

11. ✕-Sub-Float rejected.—Two patterns of Sub-Float were tried in these Experiments, viz.,—

1°. Spherical, consisting of a heavy wood Ball or a loaded copper Shell.

2°. Cross-(✕) pattern, consisting of two tin Discs placed across each other at right angles.

Comparative Experiments were made between the two patterns at an early period of the work (March 1875), and *every precaution was taken to make the Test a fair yet crucial one.*

Experiment. The Instruments to be tested (for detailed description, *see* Art. 40, 44 of 1874-75 Report) were prepared with Sub-Floats exposing same area (a 3" circle) to current. The Experiments consisted of repeated velocity-measurements *done one by one with each Instrument in turn in rapid succession*, (so as to secure close similarity of the "External Conditions",) viz.,

156 trials of each at 6' depth, in water from 6'·6 to 7'·55 deep;

all the trials being *at mid-channel*, (in which position accidental deviations to right or left affect the result least.) Full details of these trials will be found in Art. 53 to 55 of the 1874-75 Report. The general Conclusions arrived at in respect of the ✕-pattern were—

"The ✕-pattern Sub-Float has two faults, Nos. vii, viii of Art. 8, viz.,—

Fault vii. Insufficient Stability to maintain itself at constant level,

Fault viii. Liability to tilt, and thereby expose greatly reduced area to the current", ... (9).

These faults are both pretty serious, especially the latter; they could have been remedied in part by increasing the weight of the Sub-Float; this would have involved an increase in the Surface-Float, which would have destroyed one of the principal advantages of the Instrument (the use of a very small Surface-Float).

This pattern of Sub-Float was accordingly rejected, and the spherical pattern finally adopted for these Experiments.

12. Patterns adopted.—The whole of the (systematic) Subsurface work of the present Experiments was done with two Double-Floats of same pattern, viz., with spherical Sub-Float and disc Surface-Float, differing only in size and material. They will be called for shortness the

8″ *Double-Float*, or simply "8″ Ball", used from 1875 to Feby. 1876.
1⅝″ *Double-Float*, or simply "1⅝″ Shell", used from Feby. 1876 to 1879.

The 3″ wood Ball was the one first tried, and was used with satisfactory results, as far as accuracy goes, until February 1876: it was so inconvenient in practical use on account of the hygroscopic qualities of the wood (causing undue absorption of water), that it was eventually given up in favor of the 1⅝″ copper Shell, which was the only pattern in use from February 1876.

[Specimens of these Double-Floats were sent to Mr. R. Gordon (the Irrawaddi Experimenter) for inspection in 1878. In a note communicated to the author on 9th August 1878, he writes—

"Your Floats are models for analysis on a clear water regular canal"].

It will be seen by the comparison of dimensions of Double-Floats used in Modern Experiments given in Art. 18 and Abstract Table 2, that the small 1⅝″ copper Shell finally adopted in 1876, was by no means an improvement in point of accuracy on the older 8″ wood Ball, and was probably not nearly so accurate *at the greater depths*, (say below 6′,) in consequence of the relative Area (exposed to current) of the Connector compared with that of the Sub-Float being increased by the reduction in size of the Sub-Float.

[This defect unfortunately escaped notice until the Experiments were nearly over. It has been explained (Art. 8, iii) that the probability of this defect being in any way important has only quite recently attracted the attention of hydraulicians. And it is believed that this is the first Work on Experiments with the Double-Float in which this defect has been submitted to calculation (*see* Art. 18)].

In working up the Results (Chap. X—XIV) to be deduced from the Experiments, a *very large* allowance has been made for the decrease of accuracy with the depth, (Art. 9,) so that the Conclusions drawn will not be much affected.

The two patterns are described below: the details being taken up in following order, viz.,—

Sub-Float, Surface-Float, Connector, Cost, Handling, and Frames.

12a. 8″ Double-Float, (Pl. XI, 1).—*Sub-Float.* This consisted of a spherical Ball of 8″ diameter carefully turned (in a lathe) in some heavy wood—usually *Acacia*

Catechu (Hind. "Khair"), some specimens of which are specifically heavier than water—with a fine hole (*ma* in figure) bored right through it for the introduction of the Connector: after turning, it was boiled for some time in hot oil to diminish the hygroscopic qualities of the wood. Its weight was adjusted (by experiment in still water) so as to sink the attached Surface-Float almost flush with the water; if too heavy, it was lightened by boring out some of its mass from one end *m* of the Connector-hole; if too light it was loaded by countersinking lead into a hollow bored out at the other end *a* of the Connector-hole, until the adjustment was effected, but the hygroscopic character of the wood prevented any nicety of adjustment.

Surface-Float. This consisted either of a Disc of English deal about 3" diameter by about $\frac{3}{8}$" thick, or (occasionally only) of a slice of cork (part of a bung) about 2" diameter by $\frac{3}{8}$" thick.

Connector. This was a very fine brass wire (No. 30 Birmingham Wire Gauge, ·012" thick, weighing only 2·44 grains per foot): the ends of the wire were passed, one through a hole in the middle of the Surface-Float, the other through the fine Connector-hole (above-mentioned) in the Sub-Float; each end was then wound twice or thrice round a tiny splinter of wood (A, *a*), which sufficed to prevent the wire from returning through the hole.

Cost. The cost of this Instrument was about 6 annas (or 9 pence) each.

Handling. The use of a wire Connector caused special difficulties in handling. It was found essential never to let the wire fall slack; as when slack it commonly 'kinked', and the 'kinks' proved to be points of excessive weakness.

There was a small daily loss, due partly to breakage of wires, partly to difficulty in catching the Surface-Float whilst passing by the lower boat, partly to absorption of water by the Ball causing the Instrument to sink outright.

Frames. To keep the wires taut, the Instruments were kept lying stretched at full length on a rough bamboo framework (Hind. "jáfari") with rough fittings to receive the Surface-Floats at one end and the Balls at the other. There was a similar framework in each boat. The men who handled the Instruments were drilled to handle them in such a way as never to let the wires fall slack. This of course required some care, but after a little practice it was found that—delicate as the Instrument may seem from the description—it would bear a good deal of use with proper care.

12b. $1\frac{1}{4}$" Double-Float, (Pl. XI, 2).—*Sub-Float.* This consisted of a thin spherical Shell or hollow Ball of very thin sheet copper of No. 25 Birmingham Wire Gauge (= ·02" thick), formed by soldering two hemispherical shells together upon a narrow copper band (*cc* in figure) of same thickness. A small hollow was countersunk in the shell at the point marked *m*, and a small bit of brass wire brazed across it flush with the exterior of the shell to give means of attaching the Connector. A small lump of brass was cast into the shell at the part marked *a* to give additional weight and thickness. A screw-hole *ab* was bored right through the shell and the brass lump, and fitted with a brass screw *ac*, whose head when screwed home was flush with the exterior of the Ball, and inner end *c* tipped with lead sufficient to make the Ball sink in water. This gave the means of adjustment: as, by removing the screw, lead could be added or cut off as necessary. The Balls when complete weighed about 540 grains in air, and about 30 grains in water, so that the Tension of the Connector was about 30 grains.

Surface-Float. This consisted of a thin slice of a sound cork (such as is used for soda water bottles) about 1" diam., and not more than ¼" thick. The minimum reserve of buoyancy (*i. e.*, excess of buoyancy over that required for the mere support of the weight of the Connector and Sub-Float) was about 6 grains in 1876-77: this was increased to about 15 grains in 1877-79. It was kept oiled to prevent absorption of water.

Connector. This was a fine black silk thread of about $\frac{1}{125}$" thickness* in 1876-77, and $\frac{1}{80}$" thickness* in 1878-79; it was oiled at intervals to prevent absorption of water.

It was attached to the Sub-Float by tying it round the small wire *m* above-mentioned as brazed across the hollow *m* at top of Shell, and was attached to the Surface-Float by passing it through a small hole in the middle thereof, and then tying it round a tiny splinter of wood A, which prevented its return through the hole.

Cost. The cost of this Instrument was about one rupee (or two shillings) each.

Plank Trays, (*see* Pl. XI, 3). The use of such thin sheet copper and such thin silk made the Instruments somewhat delicate. To protect them from injury, and to keep the Connectors always stretched, PLANK TRAYS were made up of planking (of a light wood) about 9" wide and 1" thick, with special fittings for the Sub-Float and Surface-Float, to carry about 18 at once.

For the Sub-Floats, 18 hemispherical hollows CCCC were countersunk into each Plank near one end in four rows CCCC of 4 and 5 alternately, just large enough to receive one of the Sub-Floats. A rough movable Lid L*l*, movable about pivots at the points PP was provided sufficient to cover over the whole group of 18 Sub-Floats at once, and protect them from external injury.

For the Connectors, four "Bridges" BBBB (one for each row of holes C) consisting of strips of wood about ¾" × ¾" were screwed across the Planks near the other end B, and 4 or 5 saw-cuts (*s*) were made across each Bridge to receive the Connectors. The Connectors were led out from the Sub-Floats underneath the front of the Lid, and laid at full length along the Planks and carried through the saw-cuts, each one through a different saw-cut, so as to be kept stretched.

The Surface-Floats stood on edge on the Plank, close up to the Bridges: the distance between the centre line of each transverse row of holes C and the corresponding Bridge was made equal to the intended depth of immersion of the Sub-Float.

Thus each "Plank-Tray" held 18 Double-Floats always ready for work. This arrangement was found sufficient for their protection, and convenient in practical use.

Handling. When in actual use the full Plank-Tray was put in the Upper Boat, and a similar empty Plank-Tray in the Lower Boat to receive the Instruments as lifted out of the water. Some care was required in throwing from the Upper Boat to prevent the Connectors falling slack and getting tangled; and still more care was required in lifting out of the water at the Lower Boat to keep the Connector from being frayed against the bottom or sides of the Boat. There was a frequent small loss from fracture of the Connector from this last cause.

* These may seem surprisingly thin: they were carefully gauged by the author himself both upon new boxwood Scales, and in a Wire-Gauge, in presence of witnesses (Lieuts. S. M. Maycock, R.E., and J. H. C. Harrison, R.E.) Very few Experiments (only 28 in all) were made with the thicker Connector.

The very small Surface-Float used could not be easily seen from a distance in certain states of the light: to render it more distinct, a small pledget of cotton wool was sometimes placed on the top of the Surface-Float underneath the little splinter of wood A which retained it in its place.

13. Modern Double-Floats, (Abstr. Table 2).—The Table quoted contains a brief description of all the Double-Floats used in all the large modern Experiments, (Mississippi, Lake River,* Connecticut, Irrawaddi, and Roorkee,) with the dimensions and weights of their principal parts, and also the Areas, both actual and relative of those parts, exposed to current-action *both direct and lateral*, when in use at the maximum depth, (*i.e.*, in the use most unfavorable to each.)

The areas exposed to direct and lateral current-action (*i.e.*, to pressure and to adhesion) have been estimated as follows for each of the parts, (the calculation is only a rough one, as the published data are in many cases imperfect):—

Direct Area = Vertical projection upon a plane $\perp^r$ to the axis of the current, (10).
Lateral Area = Sum of vertical projections to *both right and left* upon a vertical plane through axis of current,
+ horizontal projection of under surface in case of Surface-Float,
+ Sum of horizontal projections of both upper and under surfaces in case of Sub-Float,..(11).

By this mode of calculation it will be seen that—

Surface-Float, Lateral Area = Horizl. projection of under surface + 2 × Direct Area,..............................(11*a*).
Connector, Lateral Area = 2 × Direct Area,..............................(11*b*).
Sub-Float, Lateral Area = Sum of horizl. projections of upper and surfaces + 2 × Direct Area,..............................(11*c*).

It will be seen (from the Table) that in all the cases—

"The Surface-Float Areas are small compared with the Sub-Float Areas, (and in some cases are really relatively negligible,)"..............................(12).

"The Connector Areas (with Connector of maximum length) are in no case less than $\frac{1}{16}$ of the Sub-Float Areas, and are in several (four) cases large fractions of the latter, and in two cases actually exceed the latter",..............................(13).

It will now be seen that the increase of area of the Connector with increase of depth is the chief source of difficulty in the Design of a Double-Float. The best Designs in this respect appear to be the—

Mississippi of 1858 (for use under 5′ depth), Connecticut, and Roorkee of 1875-6; and the worst appear to be the—

Mississippi of 1851 & 1858, Irrawaddi, and Roorkee of 1876—1878.

* The Lake River Reports were not received in time to include the description of Double-Floats used in the Table: the patterns used were apparently identical with those described for the Mississippi Experts., (*see* Lake River Report of '68, pp. 950, 951), but detailed dimensions are not given.

TITLE, Author, Place and Date of publication.	Reference to page of Original.
Report upon the Physics and Hydraulics of the Mississippi River, ... Humphreys, A. A., & Abbot, H. L.; Philadelphia, '61	224, 225
Survey of the Northern and North-western Lakes, ... [Report of Chief of Engrs., U. S. A. for '69, Appx. XA]; Henry, D. F.; Washington, '70	563
[" " " for '70-'71, Appx. AAD]; Henry, D. F.; Washington, '70	554, 555
[" " " for '70-'71, Appx. BB]; Abbot, H. L.; Washington, '70	620—631
Franklin Institute Journal, ... Henry, D. F.; Philadelphia, '71	? ?
Hydraulics of Great Rivers, ... Révy, J. J.; London, '74	4—8
On River Gauging and the Double-Float, ... [Van Nostrand's Maga., Vol. XIII, p. 89]; Robinson, S. W.; New York, '75	99—109
Discussion des expériences les plus récentes sur la distribution des vitesses dans un courant, ... [Annales des Ponts et Chaussées, Vol. X, p. 309]; Bazin, H.; Paris, '75	324—331 337—347
Theory of the Flow of Water in Open Channels, ... Gordon, R.; Rangoon, '75	21
Hydraulic Experiments at Roorkee, 1874-5, ... [Profl. Papers on Ind. Engng. Vol. IV, of '75, No. 18A]; Cunningham, A.; Roorkee, '75	14, 15
Improvement of Rivers and Harbors in the States of Connecticut, &c., [Report of Chief of Engrs. U. S. A. for '75, Appx. AA, 14]; Warren, G. K.; Washington, '75	301
[" " " " Appx. AA, 14]; Ellis, T. G.; Washington, '75	305, 306
[" " " " Appx. AA, 15]; Humphreys, A. A.; Washington, '75	369—372
Report of the Surveys and Examinations of the Connecticut River, ... [Report of Chief of Engrs. U. S. A. for '78, Appx. B, 14]; Warren, G. K.; Washington, '78	259
[" " " " Appx. B, 14]; Ellis, T. G.; Washington, '78	305—307 380—383
Hydraulics of the Irrawaddi, ... [Report on the Irrawaddi, Part III.]; Gordon, R.; Rangoon, '79	16, 17
The Mississippi as a Silt-Bearer, ... [Van Nostrand's Maga., Vol. XX, p. 218]; McMath, R. E.; New York, '79	224, 225

It must be remembered, however, that the Faults (iii, iv, v, vi, Art. 9) dependent on this are of trifling importance at small depths, and become serious only at the great depths, so that the Results obtained even by the most faulty of the Instruments (the last three) will be accurate enough at all small depths, or perhaps down to mid-depth, and will decrease in accuracy from thence to the bed. In drawing Conclusions from the observations with these Instruments, a proper allowance should be made for this.

[In respect to the present Experiments, this allowance is made in Chap. X—XIV].

14. Double-Float, CONTROVERSY.—It will be convenient to quote here the following references from modern writers to the Controversy on the Double-Float, *see* Table on last page.

CHAPTER X.

VERTICAL VELOCITY-CURVES.

***Preface.*—This Chapter contains a detailed description of the Experiments for deducing Vertical Velocity-Curves, with discussion of some of the Results. The most important Articles are Art. 1—3, 6—8, 11, 12b, 15.**

1. Vertical Velocity-Curves.—This term has already been defined (Ch. I, 9) as the—

"Curve whose ordinates are the 'forward velocities' at all points of a vertical Base-Line in any part of a channel".

The importance of these Curves to both Theory and Practice has already been explained in Ch. I, 10.

Much and continuous Experiment was accordingly devoted to obtaining data for this research. The Experiments were made at three different Sites (viz., at the Soláni Embankment and Twin Soláni Aqueduct Sites), *at several different verticals* in each Site, and at *several different depths* on each of those verticals, as shown in following Tables:—

Central Verticals.															
Soláni Left Aqueduct.				Soláni Right Aqueduct.				Soláni Right Aqueduct (Left Aqueduct closed).				Soláni Embankment.			
Table.	Serial No.	Number of Sets.	Average Depth.	Table.	Serial No.	Number of Sets.	Average Depth.	Table.	Serial No.	Number of Sets.	Average Depth.	Table.	Serial No.	Number of Sets.	Average Depth.
VII	1	20	9'.46	IX	5	16	9'.94	XV	18	4	4'.66	XVI	21	16	10'.89
VIII	2	22	8'.96	,,	6	16	9'.41	,,	19	8	4'.21	,,	22	16	7'.93
,,	3	12	7'.25	X	7	18	8'.85	,,	20	10	3'.99	XVII	23	10	7'.79
VII	4	4	5'.92	,,	8	16	8'.42					,,	24	6	7'.39
				XI	9	14	8'.32					XVIII	25	12	7'.26
				,,	10	2	8'.13					,,	26	7	6'.50
				,,	11	15	7'.75					XIX	27	16	6'.24
				XII	12	20	7'.59					,,	28	16	6'.16
				,,	13	4	7'.15								
				XIII	14	16	6'.77								
				,,	15	9	6'.22								
				XIV	16	16	6'.01								
				,,	17	3	5'.55								
TOTALS	4	58	..	..	13	165	..	..	3	22	..	..	8	99	..

Non-Central Verticals.												
Soláni Right Aqueduct.						Soláni Embankment.						
Table.	Serial No.	Position of Vertical.		Number of Sets.	Average Depth.	Table.	Serial No.	Position of Vertical.			Number of Sets.	Average Depth.
		Distance from centre.	Left or Right of centre.					Distance from centre.	Left or Right of centre.	Position.		
XX	29	41'⅜	L	16	8'·39	XXVIII	41	79'$\frac{1}{12}$	L	on middle wetted step,	16	2'·58
"	30	"	"	5	6'·96							
XXI	31	41'½	L	16	8'·45	XXVI	42	75'½	L	on last step,	16	5'·60
"	32	40'	L	16	8'·64	"	43	"	"		16	3'·61
XXII	33	"	"	16	8'·62	XXVII	44	74'½	L	close to the 4' drop wall,	5	8'·78
"	34	37'½	L	16	9'·42	"	45	"	"		6	8'·49
XXIII	35	30'	L	4	9'·55	"	46	"	"		8	8'·19
"	36	"	"	15	9'·33							
XXIV	37	"	"	18	9'·01							
"	38	30'	R	12	9'·17							
XXV	39	"	"	4	8'·85							
"	40	37'½	R	16	8'·68							
TOTALS,	12	..	..	154	..	..	6	..	..	..	67	..

This gives a Total of 46 Curves, (all Average Curves,) viz.,

28 Curves on Central Verticals—formed from 844 SETS,

18 Curves on Non-central Verticals—formed from 221 SETS,

which is believed to be a larger collection than has ever before been published.

[Ser. Nos. 1 to 4 and 12 were published in the 1874-5 Report (where they are numbered 14B, 13B, 11B, 10B, and 12B, respectively); they are reprinted now—with some modification—for completeness' sake].

2. Instruments.—The velocity-measurements were made with the following Instruments:—

Surface-velocity. SURFACE-FLOAT, same as that of the Double-Float in use.

Subsurface-velocity. DOUBLE-FLOAT, (with small surface-float,) viz.,
3" wood Ball pattern in 1875-76, (Ch. IX, 12a).
1⅝" copper Shell pattern in 1876-78, (Ch. IX, 12b).

Mean velocity. Loaded 1" wood RODS in 1874-76, (Ch. XV, 7a).
Loaded 1" Tin TUBE-RODS in 1876-79, (Ch. XV, 7c).

[The present Chapter deals solely with the figure of the Curve. The use of the "Rods" will be explained in Ch. XV].

3. Mode of research.—The mode of research proposed (Ch. I, 11) was to measure the "forward velocity" at many points of the same vertical, *e.g.*, at the surface and at many points below, thus giving the values of many ordinates of each Vertical Velocity-Curve.

4. Ordinate-system.—The Unsteady Motion of the water necessitates the making of a large number of velocity-measurements (Ch. VI, 4a) at each point to give a fair value of the Average Velocity at each. To do this with the Double-Float with anything like moderate rapidity, a considerable number of Instruments must be available *ready adjusted to the various depths* required, so that there may be no delay in adjustment to the various depths during the progress of the Field-work.

There are only two Ordinate-systems which are generally convenient, viz.—

Fractional system. The Ordinates are at convenient fractions of the whole depth, *e. g.*, at the surface, and at $\frac{1}{10}$, $\frac{2}{10}$, $\frac{3}{10}$, &c.,...... $\frac{9}{10}$ of the full depth.

Integer system. The Ordinates are ranged at convenient multiples of any convenient length-unit, *e. g.*, at every foot, yard, or mètre, &c.

The "fractional system" is the most convenient for the numerical computation attending the investigation of the figure of the Curve, and is therefore usually employed with Instruments which, like the Current-Meter and Pitot's Tube, can be set with equal convenience at any depth, each time they are used: but it is very inconvenient with Instruments used in large numbers like the Double-Float, as the ever-varying depth of water would involve a constant re-adjustment, not only of the lengths of the Connectors, but also of the positions of the "Bridges" (Ch. IX, 12b) upon the "Plank-Trays", by means of which the Connectors are kept permanently stretched. This involves such great waste of time as to be impracticable.

The "integer system" is then the only convenient one for use with* Double-Floats, as a large stock can be kept with their Connectors adjusted once for all lying ready for use at all times.

[Thus in the present Experiments the Ordinate-system for velocity-measurements upon any vertical was—

At surface, at 1′ depth, at 2′, 3′, 4′, &c., and so on at every foot of depth,

and a large stock of Double-Floats was kept always ready with their Connectors adjusted to all lengths in whole feet from 1′ to 10′ (the maximum depth of Experiment,) a set of 9 for every foot. In a hot climate it is of great importance to have the Instruments always ready for use, so as to be able to utilize the few cool hours immediately after daylight for actual Experiment without any unnecessary delay in adjusting the Instruments.

It will be seen that a Set of 90 Double-Floats were sometimes in use at once (viz.,

* It was the system actually used in all the large Modern Experiments with the Double-Float—Miss. Report, Tables on p. 230, *et seq.*; Lake River Report of '68, p. 955; Irrawaddi Report of '75, Art. 38; Connecticut Report of '78, p. 310.

9 for every depth from 1′ to 10′): the impracticability of frequent re-adjustment of so large a number will be obvious].

5. Field-work.—The mode and order of the Field-work has already been explained in Ch. VI, 12a, *q. v.*

[The velocity-measurements were always made as nearly in the order there explained, viz.,—

> "3 at surface, 3 at 1′ depth, 3 at 2′ depth, and so on in succession, usually down to the lowest* depth (in whole feet say *n* feet) attainable; lastly, 6 loaded Rods of length (l) nearly equal to the full depth (H),"

as was compatible with practical convenience. It frequently happened, for instance, that—in consequence of the Unsteady Motion of the water—the whole available stock of Instruments suited to a particular depth was cast from the Upper Boat without the requisite number (three) of GOOD FLOATS resulting fit to record. To make up the group of three velocities for that point there and then would have necessitated the considerable delay of sending the Boats to bank to exchange the Instruments from the Lower to the Upper Boat. To save this delay, the actual practice was to pass on to the next depth when the whole available stock of Instruments for any one depth was exhausted. The Instruments were thus exchanged from the Lower to the Upper Boat only after an attempt had been made to complete the requisite three at each depth. After the exchange an attempt was made to supply the missing observations in regular order as before, *i. e.*, from the surface downwards].

6. Tabulation, Series, (Tab. VII—XXVIII, and 3, 4).—Tables VII—XXVIII contain the whole of the DETAILS connected with the Vertical Velocity-Curves. The mode and order of tabulation of the SETS, and the combination used in forming them into SERIES, have been explained in Ch. VI, 12c—13a, *q. v.* These Articles include also the explanation of most of the Columns of these Tables containing the Velocity-Data. The explanation of Col. 3 (Fall of Water-Surface) will be found in Ch. VII, 2e, 9a. The explanation of the Result Columns (Nos. 7, 8) will be given in due course. A brief explanation of the whole of the Columns is given at p. 13 of the Tables themselves.

To save unnecessary looking over details, Abstracts have been prepared (Tab. 3, 4), showing the MEANS and RANGES of the principal Data and Results for each Series.

[In Tab. 3, 4 two lines are devoted to each Series; the upper shows the Means, and the second the Ranges; *see* Explanation at head of each Table].

7. Average Vertical Velocity-Curves, (Pl. XII—XVIII).—Each Diagram shows the Average Velocity-Measurements at the surface and at 1′, 2′, 3′, &c., *n* feet below the surface in clear black lines, as ordinates plotted from the Base-vertical close to which the depths are figured. The Rod-Velocities (u) actually observed are also shown in clear black lines.

Velocities *computed* from these data (not observed) as hereafter explained, viz., the—

Mid-depth Velocity ($v_{\frac{1}{2}H}$), Bed Velocity (v_H), Mean Velocity (U),

are shown *in dotted lines.*

The Mid-depth Velocity ($v_{\frac{1}{2}H}$), and Bed Velocity are plotted as ordinates from the proper points (mid-depth and foot) of the Base-vertical. The Rod-velocity (u) and

* *see* Art. 13 for explanation of case where $n > H$.

Mean Velocity (U), which do not belong to any *particular* point of the Base-vertical, are plotted as ordinates *in any convenient position* where they could be introduced without confusion.

The tips of the velocity-ordinates at the one-foot intervals, and of the mid-depth and bed ordinates are joined by clear straight lines: the irregular line so formed is the AVERAGE VERTICAL VELOCITY-CURVE given by the data, the junction lines being all *straight*, the irregularities inevitable in the Observation-Curve are clearly exhibited, without the introduction of any bias of the draughtsman's hand (which inevitably results where free-hand curves are drawn).

The tips of the Mean Velocity- and Rod-velocity-ordinates only do not lie on the Curve itself. They are indicated by arrow heads, and also by a (clear) vertical drawn right across the curve. These lines have in each case important physical meanings,—

1°. *Rod-velocity.* The vertical line is a "picture" of the Rod itself, indicating (by its position) the position of the Rod in the Curve, and also (by its length) the length of the Rod. The foot of the Rod is indicated by a slight horizontal scratch intended merely as a conventional way of showing (without possibility of mistake) the end of the vertical line.

2°. *Mean Velocity.* The rectangle included between the vertical line in question, and the surface- and bed-velocity ordinates and the Base-vertical is equal in area to the area contained between the Curve itself and the three last lines.

In the case of Velocity-Curves for a Non-Central Vertical (Pl. XVI—XVIII), Cross-Sections (of the whole or of part of) of the SITE are also given, showing the exact position of the Experimental verticals in the several Sites.

The dotted Curves show the Velocity-Parabolæ obtained as explained in Ch. XI, 7. The position and magnitude of the maximum velocity line (or parabola-axis) is indicated by a pair of arrows, one pointing to the vertex of the curve, and one (marked V) to the origin of the line on the Base-Vertical.

7a. *Velocity-ordinate exaggeration.*—The depths shown on the Base-Vertical are all on the scale of 10′ to an inch; but the velocity-ordinates are all on the scale of 1′ per second to an inch. Scales of depth and velocity are of course not really comparable: but it is clear that—adopting the second as the time-unit—the velocity-ordinates are *exaggerated ten times.* This exaggeration has been necessary to show the curvature of the curves in any distinct manner. It will be seen, therefore, at once that all the curves are really very flat.

8. Properties of the Curves.—A mere cursory examination of the Curves (Pl. XII—XVIII) will show at once the following prominent properties, in addition to those (common to all velocity-curves) discussed in Ch. VI, 15, from which it will be remembered that in this discussion, Curves derived from numerous SETS of data are entitled to more weight than Curves depending on only a few Sets (in consequence of their better approximation to really Average Curves).

"The Curves are generally everywhere convex down-stream, (except of course near an irregular margin, (*e.g.*, in Ser. 44 to 46, Pl. XVIII)",........................ (1).

"The maximum velocity is usually below the surface",........................... (2).

"The line of maximum velocity sinks (in a rectangular channel) from the centre towards the banks, and is about mid-depth at the banks", [*see* Pl. XVII,]......(3).

"The line of maximum velocity lies near the surface for verticals near and over the banks of a channel with sides laid out in steps",............................(4).

"The velocities near the bottom are generally the least",...........................(5).

"The mid-depth velocities are generally greater than the means",..............(6).

"The differences between the velocities in any one curve are all small quantities (compared with the velocities themselves)",.. (7).

"The Curves are all decidedly flat",............ ....(8).

"The flatness of the Curves decreases (in a rectangular channel) from the centre towards the banks", [*see* Pl. XIII, XVI, XVII,] ....(9).

It is worth while noticing here, before entering into a detailed discussion, that the general features above described agree closely with the Results for similar cases of Mr. Bazin's small scale Experiments in rectangular channels.

[Compare Pl. V, XVIII to XXI, XXIII, XXVI of the Bazin Expts. Atlas with the present Plates].

9. Notation, (Pl. XX, 3).—In what follows, the following notation will be used :—

z = depth of any point below surface.

z' = depth of immersion of Sub-Float, (always $< z$).

Z, h_o, h = depths of lines of Max. Velocity, Mean Velocity, Rod-Velocity.

H = full depth.

ζ = proportionate depth (*i. e.*, Z $\div$ H) of max. velocy.-line.

v, v_z = velocity at depth z.

v' or v_z' = velocity of Double-Float with Connector-length z, in Chap. X.

v' or v_z' = ordinate of velocity-parabola at depth z, in Chap. XI.

v_o, $v_{\frac{1}{2}H}$, v^H = surface-, mid-depth-, bed-velocity.

V, U, u = Maximum-Velocity, Mean Velocity (past the vertical), Rod-velocity.

p = parameter of velocity-parabola.

m = (reciprocal of p) = $1 \div p$.

The following scheme shows the correspondence of depths and velocities :—

Depths, z = 0, 1, 2, 3,......... Z, $\frac{1}{2}$H, h_o, h, $\frac{3}{4}$H,.........($n-1$), n, H.

Velocities, v or v_z = v_o, v_1, v_2, v_3,..........V, $v_{\frac{1}{2}H}$, U, u, $v_{\frac{3}{4}H}$,......... v_{n-1}, v_n, v_H.

10. Instrumental Distortion of Curve, (Pl. XIX).—Errors of velocity-measurement inherent in the Instruments employed will of course affect the apparent figure of the Curve derived from them in a definite way. An attempt will now be made to estimate the general effect of the known sources of error (Ch. IX, 8) due to the use of the Double-Float. They will be taken in the order of Ch. IX, 8, viz.—

i. Lateral Deviation of Sub-Float.

ii. Surface-Float Resistance.

iii. Connector-Resistance.

iv. Lift of Sub-Float due to lateral deviation.

v. Lift of Sub-Float due to its Lag or Lead.

vi. Lift of Sub-Float due to curvature of Connector.

vii. Instability of Sub-Float.

The disturbing causes Nos. i and vii being uncertain and irregular in their operation, (*see* Ch. IX, 8,) their effect on the Velocity-Curve is irregular, and does not admit of estimation. For each of the remaining five causes (Nos. ii to vi) of distortion of the curve, two principal cases will arise, viz.—

CASE (*a*). Greatest Velocity at the surface, (Pl. XIX, ii*a*, iii*a*, v*a*, vi*a*).

CASE (*b*). Greatest Velocity below the surface, (Pl. XIX, ii*b*, iii*b*, v*b*, vi*b*).

It will be seen (from what follows) that the Errors are cumulative in Case (*a*), and partly compensatory in Case (*b*). In the Diagrams, (Pl. XIX,) the " True Curve " is shown by a clear line, and the " Observation-Curve " (affected by each Error singly) by a dotted line: the direction of displacement of the points P on the true Curve to their new positions *p* on the Observation-Curve is shown by the short lines as P*p* between the Curves. In the five Diagrams of Case (*b*), BC is a vertical line, so that *c*C is a velocity equal to the surface-velocity *b*B.

The Errors being all supposed small, their effects may—by the principles of Infinitesimals—be separately estimated, and afterwards superimposed.

10, ii. *Surface-Float Resistance*, (Pl. XIX, ii*a*, *b*).—The action of the surface water on the Surface-Float necessarily accelerates or retards the Sub-Float according as the surface-velocity is > or < the velocity of the stratum in which the Sub-Float moves, so that—

"Each subsurface velocity-measurement (v') exceeds or falls short of the real velocity (v) according as the surface velocity is greater or less than the latter",..(10),

or, more briefly, "$v' > = < v$ according as $v_0 > = < v$,"........ ....(10*a*),

from which it is clear also that if $v_0 > < v'$, then *à fortiori* $v_0 > < v$, so that this Result may also be expressed in the following form more convenient for future discussion, viz.—

$$v' > = < v \text{ according as } v_0 > = < v', \qquad (10b).$$

It is easy to see in a general way that this error always produces a general flattening of the true curve, for all subsurface velocities which exceed the surface-velocity are under-estimated, and those which are less than the surface-velocity are over-estimated. This is clearly shown in Pl. XIX, ii*a*, *b*.

10, iii. *Connector-Resistance*, (Pl. XIX, iii*a*, *b*).—It is easy to see that the Connector tends to accelerate or retard the Sub-Float according as the curved form into which it is thrown by the current-pressure on it is convex or concave down-stream (*see Fig.* iiia_1, b_1, b_2, b_3), or according as its curvature is similar to or opposite to that of the Velocity-Curve itself. Hence—

CASE (*a*). *Greatest Velocity at the Surface, Fig.* iii*a*—

"The Sub-Float lags (*Fig.* iii*a*), the Connector is curved like the Velocity-Curve and accelerates the Sub-Float. The Observation-Curve lies everywhere outside the true Curve, and the Error *increases with the depth*", ...(11*a*).

CASE (*b*). *Greatest Velocity below the surface, Fig.* iii*b*—

"From B to A the Sub-Float leads, the Connector is curved in the opposite direction to the Velocity-Curve (*Fig.* iiib_1), and retards the Sub-Float. Below A the upper and lower parts of the Connector will be curved in opposite directions, (*Fig.* iiib_2,) down to some point Q between A, C, for which the Total Acceleration and Retardation on the Connector itself will just balance. From B to Q the Observation-Curve lies wholly within the true Curve; at Q the Curves cross, and below Q the Connector is curved like the Velocity-Curve, (*Fig.* iiib_3,) and accelerates the Sub-Float, and the Observation-Curve lies without the true Curve, and the Error *increases with the depth*",...(11*b*).

[The position of Q is unknown, but there is some reason to expect that its depth below the surface is about ⅓ of the depth of C].

Thus the general effect is a simple *horizontal translation* of each point P of the true Curve to the new point *p* of the Observation-Curve.

10, iv, v, vi. LIFT OF SUB-FLOAT, (Pl. XIX, v*a*, *b*; vi*a*, *b*).—It has been explained (Ch. IX, 10) that three distinct causes, viz.—

iv, Lateral Deviation; v, Lag or Lead of Sub-Float; vi, Connector-curvature, combine to lift the Sub-Float, so that the velocity-measurement (v') effected at the real depth (z'), corresponding say to the point P, (*Fig.* v, vi,) is attributed to the *lower* point *p* corresponding to the greater depth (z) indicated by the length of the Connector.

The result is that a general *depression* of the velocity-ordinates takes place, accompanied by a *vertical* depression of each point P of the true Curve to the new point *p* of the Observation-Curve.

CASE (*a*), *Figs.* v*a*, vi*a*. *Greatest velocity at the surface*—

"The Observation-Curve lies wholly without the true Curve, and the Error due to causes v and vi increases with the depth, because the causes (Nos. v, vi) increase with the depth",..(12*a*).

CASE (*b*), *Figs.* v*b*, vi*b*. *Greatest velocity below the surface*—

"The Observation-Curve lies within the true Curve from B to A, and crosses it just below A (where the two Curves coincide). Below A the Observation-Curve lies everywhere outside the true Curve, but in different degrees under each cause separately"—as follows,..(12*b*).

Cause iv. "The precise variation is uncertain",...............................(12*c*).

Cause v. "The 'Lead' of the Sub-Float decreases (*Fig.* ii*b*) from A to C, and vanishes at C: hence the Lift of the Sub-Float caused thereby decreases from A to C, and the Observation-Curve (*Fig.* v*b*) lies very near the true Curve from A to C, and coincides with it at C; below that point the Error increases with the depth, because the 'Lag' of the Sub-Float increases",.....................................(12*d*).

Cause vi. "Below the point A, (*Fig.* vi*b*) the Error increases with the Depth, because the cause increases",..(12*e*).

11. **Resultant Error,** (Pl. XIX, 5*a*, *b*).—The several partial errors,

being all supposed small, may be superimposed by the principles of Infinitesimals. It will be seen by what precedes that—

"The Surface-Float Action, and Connector Action cause error of same kind, viz., horizontal displacement, and in same direction except in the small space QC in Case (*b*)", ...(13).

"The Lift of Sub-Float due both to its Lag or Lead and to Connector Action cause error of same kind, viz., vertical displacement, and in same direction except through the space AC in Case (*b*)," ....(14).

Combining the several partial Errors, it is seen—

CASE (*a*). *Greatest velocity at the surface*—

"The partial errors are cumulative, the Observation-Curve lies wholly without the true Curve, and the Resultant Error increases with the depth", ...(15*a*).

CASE (*b*). *Greatest velocity below the surface*—

"From B to A the partial errors are cumulative, from A to C they are partly compensatory, and below C they are cumulative. The Observation-Curve lies whlloy within the true Curve from B to a certain point Q between AC, crosses it at Q, and below Q lies wholly without it; also below Q the Resultant Error increases with the depth", ...(15*b*).

[The position of the point Q is of course dependent on the relative proportions of the Errors between A, C. The point Q is certainly below A, because near to A the Errors ii and iii displace directly inwards, whereas Errors v, vi displace only slightly outwards].

It is easily seen also that—

"The combined Errors produce in all cases a general flattening of the whole Curve," ....(16).

and therefore,

"The whole of the Observation-Curves (Pl. XII to XVIII) are too flat, especially near the bed, where the velocities are all exaggerated", ...(16*a*).

In drawing Conclusions from these Curves, this Result must be carefully borne in mind. It may also be remarked, as in Ch. IX, 9, that of all the several causes of distortion of the curve, the Connector-Resistance is the most important near the bed in deep channels.

12. **Mid-depth Velocity** ($v_{\frac{1}{2}\text{D}}$), (Pl. XX, 1).—This quantity has acquired quite an unusual importance from the suggestion in the Mississippi Report (p. 295) to use it as an approximation to the MEAN VELOCITY past a vertical. This use will be discussed in Ch. XIV, 10.

Present Experiments. It has been explained (Art. 4) that convenience in use of the Double-Float requires the velocity-measurements to be made *at constant depths* (such as at 1′, 2′, 3′, &c., below the surface); the velocity-measurements in these Experiments, therefore, *could not be made at the real mid-depth* point (which rises and falls with every rise and fall of water-level). An approximation, believed to be close enough for the present purpose and easy of application, was obtained

by simple interpolation by proportional parts between the velocity-measurements next above and next below the mid-depth point.

Thus, let r, $(r + 1)$ be the integers* next less and next greater than $\frac{1}{2}$H.

v_r, v_{r+1} the velocity-measurements next above and next below mid-depth ($\frac{1}{2}$H).

Then it is clear from Pl. XX, 1, that, *supposing the curve in the neighborhood* of the mid-depth to be a straight line, the approximate value of $v_{\frac{1}{2}H}$ is given* by either of the formulæ,—

$$v_{\frac{1}{2}H} = v_r - (\tfrac{1}{2}H - r)(v_r - v_{r+1}) \text{ or } = v_{r+1} + (\overline{r+1} - \tfrac{1}{2}H)(v_r - v_{r+1}) \ldots\ldots\ldots (17).$$

The approximate values of the mid-depth velocity were found by these formulæ *separately for every* SET of Subsurface work in these Experiments, and are shown in Sub-Col. $v_{\frac{1}{2}H}$ of the Tables VII—XXVIII. These may be called Rough Mid-depth Velocities. The mean at the foot of the $v_{\frac{1}{2}H}$ Sub-Column in each SERIES is the AVERAGE MID-DEPTH VELOCITY-MEASUREMENT of that SERIES nearly freed from Observers' personal equation (*see* Ch. VI, 13).

[It is clear from the figure that, since the Average Vertical Velocity-Curves are everywhere convex down-stream (Art. 8),

"The above approximate value of the Average Mid-depth Velocity is usually too small", ...(18).

12a. *Mid-depth Velocity not constant.*—One of the most startling Conclusions in the Mississippi Report is that the Mid-depth Velocity is sensibly constant (for the same vertical with constant water-level) under all changes in the "External Conditions" (wind, surface-slope, &c.)

After coming to the Conclusion that the ratio of the Mid-depth Velocity to the Mean Velocity past a vertical is sensibly constant (for the same vertical with constant water-level) under all changes of wind, it is stated (p. 294 of Report)—

"The constancy of this ratio necessarily implies that the velocity of the mid-depth layer of water in a river is not affected by any changes that take place in the direction and force of the wind, whatever their extent may be, even from a calm to a hurricane",

and again, after explaining the disturbing effect of the wind on the velocities in general to be such (p. 295 of Report) that—

"the velocity of the mid-depth layer of the fluid mass must remain unaffected";

it is added,—

"This being established, it follows that the mid-depth velocity is independent of the position of the axis, and therefore is not affected by irregularities of the bottom".

This broad generalization evidently rests upon the *tacit assumption*†—(of which, however, no experimental evidence is given)—

"The Discharge and Mean Velocity past a vertical are assumed sensibly constant for the same vertical with same water-level (*see* p. 295† of Report) under all changes in the External Conditions",

in addition to the previous assumption of the constancy of the ratio of the Mid-depth Velocity to the Mean Velocity past the vertical: and the Conclusion therefore

* The velocity-measurements being (Art. 4) at *successive whole feet* of depth. An obvious modification is required for any other interval.

† This statement is of course *not a quotation*.

rests—*not upon a comparison of the actual Mid-depth Velocity-measurements themselves*, but *upon the supposed constancy of the two quantities* mentioned, *i.e.*, *on very indirect evidence*, which is itself doubtful.

[This comparison from the Mid-depth Velocity-measurements themselves cannot be done from the Results *published* in the Mississippi Report: for these Results are all AVERAGE VELOCITIES without details, and hardly any two of the SETS appear to have been done upon one and the same vertical, at nearly the same water-level and same Mean Velocity of the River, so that they are probably not inter-comparable].

However this may be in the case of Great Rivers, it is certainly not a general law of fluid motion in Open Channels. The present Experiments show this amply;—

A mere glance down Col. 8 of any of the Tables VII—XXVIII will show at once that the Mid-depth Velocity-measurements in successive SETS of the same SERIES (with a maximum variation of water-level of only about ·3′) vary greatly in magnitude. The "Range" or amount of this variation is shown at foot of Col. 8 for each SERIES along with the Average Mid-depth Velocity ($v_{\frac{1}{2}H}$). An Abstract of these two Results—the Average Mid-depth Velocity and its "Range"—in each SERIES is given in the $v_{\frac{1}{2}H}$ Sub-Column of Abstract Tables 3, 4, in which they can be compared at a glance. The "Ranges" will be seen to be frequently *pretty large fractions* of the whole quantity. The cases in which the Ranges are the *largest fractions* of the whole quantity are shown below, separately for the central and non-central verticals at each Site.

SITE.	SOLANI AQUEDUCT.				SOLANI EMBANKMENT.	
	Left Aqueduct.	Right Aqueduct.				
		L. Aqued. open.		L. Aq. closed.		
Position of Vertical of Experiment.	Central.	Central.	Non-central.	Central.	Central.	Non-central.
Reference, .. Serial Number,	1	9	29	19	28	42
Reference, .. Number of Sets,	20	14	16	8	16	16
Average Mid-depth Velocity ($v_{\frac{1}{2}H}$),	4·04	4·25	2·70	5·85	2·67	3·32
Range, *i.e.*, excess of greatest over least value, ...	·40	·84	·38	·69	·32	·63
Range Percentage,..	9·9	19·8	14·1	11·8	12·0	19·0

This variation might be supposed due wholly to the change of "External Conditions" between the different SETS of the same Series. But there are many Cases in the Tables in which several SETS were done *in succession*, *in a perfect calm*, *with steady water-level*, and nearly steady Surface-Fall, *see* SER. 23, 27, &c., and yet the variation is *well marked in those cases* also.

But in two of the special Experiments (*see* Nos. 1, 2 of Tab. LXXIV) detailed in Ch. VI, 3b as showing Unsteady Motion, a large number of velocity-measurements were made *in as rapid succession as possible*, viz.,

48 with the Double-Float at 5′ depth in 9′·75 of water,

12 with Moore's Current-Meter at 5′ depth in 9′·65 of water,

sufficiently near the mid-depth to test this question. In these two Cases also the Range percentages are pretty large (16·9 and 12·6 per cent.)

All this evidence together points most strongly to the Conclusion that—

"The mid-depth velocity of every vertical is subject to great and rapid variation from instant to instant",..(19).

This is no doubt a consequence of the general state of UNSTEADY MOTION.

12b. *Comparative Range of Mid-depth Velocity.*—On comparing the "Ranges" of the three Average Velocities, viz., at Surface, Mid-depth, and Bed throughout all the SERIES, (which can easily be done in the Abstract Tables 3, 4, where they are all brought together,) it will be seen that the Range of the Average Mid-depth-Velocity is *generally* less than that of the Average Surface and Bed-Velocities. This can be seen at once in Table below, which shows the total number of SERIES in which the Range of the former is less or greater than the Ranges of the latter.

SITE.	SOLANI AQUEDUCT.				SOLANI EMBANKMENT.		Total.
	Left Aqueduct.	Right Aqueduct.					
		L. Aqued. open.		L. Aq. closed			
Position of Vertical of Experiment.	Central.	Central.	Non-central.	Central.	Central.	Non-central.	
Total Number of SERIES: Range of $v_{\frac{1}{2}H}$ < Range of v_0,	2	11	10	3	4	3	33
Range of $v_{\frac{1}{2}H}$ > Range of v_0,	2	2	2	0	3	3	12
Range of $v_{\frac{1}{2}H}$ ≦ Range of v_H,	4	11	11	2	8	6	42
Range of $v_{\frac{1}{2}H}$ > Range of v_H,	0	0	1	1	0	0	2

This shows that—

"The variability of the Mid-depth-Velocity is generally less than that of the Surface- and Bed-Velocities",..(20).

This is sufficiently accounted for by the disturbing effect of the wind on the Surface-velocities, and of the unevenness of the bed on the Bed-velocities.

13. **Bed-velocity** (v_H), (Pl. XX, 2).—Velocity-measurements can not

of course be done upon the actual bed-level, so that some approximate estimation is unavoidable.

Present Experiments. The velocity-measurements made at the one-foot intervals, and especially those near the bed, enable an approximate value of the bed-velocity, (v_H) to be found. An approximation quite close enough for the present purpose, and easy of application was used: two cases occur according as $n <$ or $>$ H.

Case i. It will be remembered that the real depth of immersion (z_n') of the Sub-Float was (Ch. IX, 9a) always less than the nominal depth (z_n) indicated by the length (n feet) of the Connector, so that it sometimes* happened that this *nominal depth* (n feet) of the lowest velocity-measurement *slightly exceeded the full depth* (H) on the vertical. In this case this lowest velocity-measurement (v_n) itself seems the best *simple* approximation to the bed-velocity.

Case ii. But when the nominal depth (n feet) of the lowest velocity-measurement was less than the full depth (H) on the vertical—as was the usual case—the best *simple approximation* obtainable seems to be that given by simple proportional interpolation, which is equivalent to supposing the Vertical Velocity-Curve to be a straight line near the bed, (*see* Pl. XX, 2.)

The actual approximations to the Bed-velocity (v_H) used were then—

Case i, $n >$ H, $v_H = v_n$, ...(21*a*).

Case ii, $n <$ H, $v_H = v_n - (H - n)(v_{n-1} - v_n)$,..............................(21*b*).

In both Cases the approximate value is too great, because the actual velocity-measurements v_n, v_{n-1} were effected in stream-lines at a depth really $<$ H in the first case, and really $< n$, and $(n - 1)$ in the second case, *i.e.*, in stream-lines moving quicker than the stream actually on the bed in the first case, or at the depths $n, (n - 1)$ in the second case. In the second case the Bed-velocity is further over-estimated by the mode of calculation, for the Figure shows that, since the Average Vertical Velocity-Curve is everywhere convex down-stream (Art. 8), the calculated velocity-ordinate extending up to the straight line drawn through the tips of the two lowest ordinates is $>$ than the real ordinate of the Curve. Hence—

"The approximate Average Bed-Velocity as above found is usually too large", ...(22).

The approximate values of the Bed-velocity were found from the above formulæ for every Set of Subsurface work in these Experiments, and are shown in Col. 6 of Tab. VII to XXVIII. These may be called "Rough Bed-Velocities". The mean at the foot of Col. 6 of each Series is the Average Bed-Velocity of that Series nearly freed from Observers' personal equation (*see* Ch. VI, 13).

13a. *Bed-velocity irregular.*—In many of the Diagrams a curious flattening of the Observation-Curve is noticeable near the bed: so that the decrease of velocity *appears* to be less rapid near the bed than at some height above. In *one* case indeed (Ser. 6, Pl. XIII), the Bed-velocity appears to be actually slightly *greater* than the velocities immediately above. From the inherent difficulty of velocity-measurements close to the bed, the Bed-velocities are of course the worst determined of the whole, (Ch. IX, 9,) so that it seems doubtful whether any Conclusions can fairly be drawn from the (occasional) apparent irregularity noticed.

* *See* Ser. 5, 14, 22, 30, 37, 39 for instances of this. The greatest amount occurs in Ser. 14, in which the nominal depth is $n = 7'$, whilst the real full depth $H = 6'\cdot 63$ in six cases.

In some of the Irrawaddi Curves this feature seems to be prominent and persistent: thus, *see* Irrawaddi Report of '75, Art. 45—

"taking the series of groups (series 25), * * * * * * out of a total of 32, fully three-fourths of the curves† have the greatest velocity at or near the bottom".

Mr. Gordon deduces from this and other instances, that this feature (greatest velocity near the bed) is the *ordinary arrangement in very deep water*. He infers, Art. 45, 46, that with increased depth of water,

"the lower velocities tend to increase relatively to the upper",

and endeavors to explain this as due to the increase of pressure with increase of depth; thus (Art. 49)—

"As the depth increases so does the pressure, and on the pressure theory, the lower layers also tend to increase their velocity, and do so, as shown by the observations, to such a degree, that finally they become the greatest in very great depths".

Admitting the facts, (*i. e.*, accepting the results of observation) the proposed explanations *must nevertheless be rejected*. For, it is clear that the fluid pressure at any given depth being the same in all directions, the "forward pressure" at any point (which is here said to tend to increase the "forward velocity") is just balanced by the "backward pressure" at same point. The mere increase of pressure with depth (*i. e.*, along a vertical) is of no avail in producing increase of "forward velocity": in order that pressure might be effectual in producing increase of "forward velocity" in the manner proposed, it would be necessary that there should be an *excess* of "forward pressure" over "backward pressure" at same point of each vertical, and that this *excess should increase* with depth.

The feature in question is certainly difficult to account for, if really the *ordinary arrangement in very deep water*, but its habitual occurrence is by no means clear. Its frequent occurrence—even to the extent of existing in three-fourths of the† curves of a *single* series of groups, (as represented,) is sufficiently accounted for by the Unsteady Motion of the water, from which it will certainly often happen that the deepest velocities are measured *about their maximum phase*, and therefore bear unusally high values. This is indeed of frequent occurrence in the present Experiments, (*see* Tab. VII—XIX), but it disappears usually in the Average values of the velocities; (Ser. 6, Tab. IX being the only exception.)

14. Mid-depth- and Bed-Velocities, BEST APPROXIMATION.—The best approximations obtainable from the data—the velocity-measurements at one foot intervals—would of course have been those given by the principles‡ of the Calculus of Finite Differences, viz.—

$$v_H = v_0 + \tfrac{1}{2}H \, . \, \Delta v_0 + \frac{\tfrac{1}{2}H \, . \, (\tfrac{1}{2}H - 1)}{1 \, . \, 2} \, . \, \Delta^2 v_0 + \dots + \frac{\tfrac{1}{2}H \, . \, \tfrac{1}{2}(H-1) \dots (\tfrac{1}{2}H - n + 2)}{1 \, . \, 2 \dots\dots (n-1)} \, . \, \Delta^{n-1} v_0, \dots (23).$$

$$v_H = v_0 + H \, . \, \Delta v_0 + \frac{H(H-1)}{1 \, . \, 2} \, . \, \Delta^2 v_0 + \dots\dots \frac{H \, . \, (H-1) \dots\dots (H - n + 2)}{1 \, . \, 2 \dots\dots (n-1)} \, . \, \Delta^{n-1} v_0, \dots\dots (24).$$

[To have applied these to each of the SETS (565 in number) in these Experiments, would have been an immense labor: the Results did not seem of sufficient import-

† The "curves" meant are probably what are styled in this Work "Rough Curves", (not "Average Curves",) which therefore carry little weight.

‡ *see* Boole's Finite Differences, Chap. III, Art. 2.

ance to render it worth while incurring the labor. The approximations used (above) are believed to be sufficient for the purpose].

15. **External Conditions.**—A comparison of all the Curves on one vertical—

Nos. 1–4, (Pl. XII); 5–17, (Pl. XIII); 18–20, (Pl. XIV); 21–28, (Pl. XV); 29, 30, (Pl. XVI); 32, 33, (Pl. XVI); 35–37, (Pl. XVII); 38, 39, (Pl. XVII); 42, 43, (Pl. XVIII); 44–46, (Pl. XVIII),

will show at once that—

"Any *marked* increase or decrease of any primary velocity (*e.g.*, surface, maximum, or mean) is accompanied on the whole by increase or decrease of the velocities at all points of the same vertical",..(25*a*),

though the increase or decrease is by no means similar at similar points.

The same thing may be seen from Pl. XXI, XXII, whereon five of the primary velocities (V, v_0, v_H, U, u) have been plotted as ordinates to the Average Depths (H) as abscissæ for each of the 28 Vertical Curves on Central Verticals (Ser. 1—28), all the Results of one Series being plotted on one ordinate. It will be seen that as before —

"The primary velocities past any one vertical vary roughly together",........(25*b*).

It will, therefore, probably suffice to endeavor to trace the dependence of one only of the primary velocities upon the External Conditions, and for this purpose it seems best to choose the Mean-Velocity (U) past the vertical as being the one of most practical importance. This discussion is deferred to Ch. XIV, 11.

CHAPTER XI.

VERTICAL VELOCITY-CURVE FIGURE.

Preface.—In this Chapter the mode of finding the MOST PROBABLE VELOCITY-PARABOLA is fully detailed (Art. 1—9e), and the parameter-variation is discussed, (Art. 10—11c). This research is entirely of theoretical interest. The most interesting Articles are Art. 1, 2, 3, 3e, 4, 5, 7, 7b, 8, 10, 11c.

1. Vertical Velocity-Curve, FIGURE.—The investigation of the geometric figure of the Vertical Velocity-Curve is a very delicate inquiry, but at the same time one of the highest interest. Unfortunately the primary observation-data, viz., the velocities themselves, are *not very good data for the purpose* (though the best as yet available). Thus, the *slope* of the curve at any point depends not on the velocities themselves, but only on the difference between the velocities at successive points, which difference (Δv) is always small compared with the velocities (v) themselves. Again, the figure of the curve depends on its curvature, and this depends not even on the last mentioned small differences (Δv), but on the differences between them, *i. e., on the second differences* ($\Delta^2 v$) between the primary velocities, which are always (not small, but) *minute quantities compared with the primary velocities,* (never exceeding a few hundredths of a foot per second in the present Experiments,) and therefore within the limits of probable errors of the primary Average Velocity data. So that in fact a very small error in the original average velocity-measurements (which may be of no importance whatever as regards the velocities themselves) *affects the figure of the curve enormously,* (unless it be a constant error affecting all the velocities alike, which would be of course eliminated in taking the differences). The delicacy of the investigation will thus be understood.

[If the *differences* (Δv) of the velocities at successive points were susceptible of *direct* measurement—independently of the velocities themselves—these would be the best data for the purpose].

1a. Figures proposed.—Various curves have been proposed by different Experimenters as representing the results of Experiment with sufficient approximation. The views of some of the older Experimenters (extracted from the Mississippi Report, pp. 204—206) are given below.

Name.	Epoch.	Place or River.	Figure assigned.
Gerstner, ..	?	?	Ellipse.
Woltmann, ..	1791-99	near Cuxhaven,	Parabola, with vertical axis, vertex below bed.
Eytelwein, ..	1801-20	?	Oblique straight line.
Funk, ..	1803-06	Weser,	Logarithmic.
Defontaine, ..	1820-33	Rhine,	Pair of oblique straight lines.
Raucourt, ..	1824-26	Neva,	Ellipse, major axis vertical, vertex below bed.
Baumgarten,	1837-46	Garonne & Rhine	Hyperbola.

The views of the more modern Experimenters and writers are given below.

Name.	Epoch.	Place or River.	Figure assigned.	Reference to originals, [for Titles, see Ch. I, 21].
Boileau, ..	1844-54	Metz,	Parabola, axis horizontal,	P. 307.
Humphreys and Abbot,	1851-59	Mississippi, &c.,	Parabola, axis horizontal,	P. 234.
Bazin, ..	1855-62	Burgundy Canal	Parabola, axis horizontal,	P. 228.
Henry, ..	1867-69	Niagara, &c.,	Ellipse, minor axis at or above surface,	P. 594 of '69 Report.
Révy, ..	1871	La Plata, &c.,	Oblique straight line,	P. 41.
Ellis, ..	1871-74	Connecticut,	None assigned,	..
Gordon, ..	1872-79	Irrawaddi,	None assigned,	..
Cunningham,	1874-79	Ganges Canal,	Parabola, axis horizontal,	Ch. XI.
Dupuit, ..	1863	[By theory, no Experiments]	Parabola of high order, axis horizontal,	P. 18.
Moseley, ..	1872	" "	Exponential,	P. 35, Vol. XLIV.

This diversity of figure assigned by the older Experimenters results partly from the want (in those days) of a sufficient number of Experiments, and partly from the flatness of the curve, which is in all cases so flat, that probably not one but many curves of many totally different families could be fitted to the observation-curves with an approximation quite within the limits of the probable errors in the values of the Average Velocities.

It will be seen that Messrs. Henry, Moseley and Révy are the only modern writers in distinct opposition to the use of the parabola. In an unpublished communication of 15-7-'76 to the present author, Mr. Révy writes that he—

"considers all attempts to parabolise or otherwise express the movement of water by curves of conic sections to be a mistake".

Nevertheless the weight of opinion of modern Experimenters clearly inclines now to the adoption of the parabola. Mr. Bazin writes thus in 1875 ("Discussion, &c.," p. 309)—

"Hydraulicians, however, admit generally now-a-days that the velocities on one and the same vertical vary as the ordinates of a parabola".

In the uncertainty above noticed as inherent in the question, in consequence of the flatness of the curve, it is permissible to choose for trial any curve (out of the many which would possibly fit the observation-curves with equal approximation) which admits of simple calculations in its determination. Now of all curves the simplest in

determination is the common parabola. It is presumably this which has led latterly to its selection for trial in preference to others.

2. Velocity-Parabola.—The parabola which nearly coincides with a Vertical Velocity-Curve will be called for shortness a VELOCITY-PARABOLA. The fundamental property of the parabola may be expressed—

"Square of ordinate (measured from axis) = Parameter × Abscissa (measured from tangent at vertex along the axis)",........ ....(1).

With the notation of Ch. X, 9, *q. v.*, and referring to Pl. XX, 3, it follows by the above (1), for any point P on the curve,

PN^2 = parameter × AN ; or $(Pn - Nn)^2$ = parameter × $(aA - pP)$, ... (1*a*),

whence the final equation of the curve is,—

$$(z - Z)^2 = p\,(V - v), \qquad\qquad (2),$$

also, writing $z = 0$, so that $v = v_0$ in the above, there results,

$$Z^2 = p\,(V - v_0), \qquad\qquad (3),$$

whence, by subtraction, $\quad p\,(v_0 - v) = z^2 - 2Zz, \qquad\qquad (4),$

a form sometimes more convenient for discussion.

3. Parabolic Elements, (Z, V, *p*).—The primary "parabolic elements" in the equation of the velocity-parabola are *only three*, viz., the maximum velocity (V), the position of that line defined by its depth (Z) beneath the surface, and the parameter (p). These being found, the parabola is definitely determined.

To determine these three quantities, three data, *e.g.*, three velocity-ordinates (v_z) at different known depths (z) beneath the surface, are of course necessary and sufficient.

3a. *Three-velocity Case.*—When only three velocity-measurements are to be made, the most convenient (for subsequent calculations) are the following :—

Surface-velocity (v_0), and two velocities $v_{\frac{1}{2}l}$, v_l at depths $\frac{1}{2}l$, and l,

and the calculation is as follows :—

Substituting these known values ($v_0, v_{\frac{1}{2}l}, v_l$) of v in the equations (3, 4) of the curve, with the corresponding values ($0, \frac{1}{2}l, l$) of z, there result the following :—

$$p\,(V - v_0) = Z^2, \qquad p\,(v_0 - v_{\frac{1}{2}l}) = \tfrac{1}{4}l^2 - lZ, \qquad p\,(v_0 - v_l) = l^2 - 2lZ,$$

solving which for p, Z, V, it is easily found that—

$$p = \frac{l^2}{2\left\{2v_{\frac{1}{2}l} - (v_0 + v_l)\right\}} \qquad\qquad (5a).$$

$$Z = \frac{4v_{\frac{1}{2}l} - (3v_0 + v_l)}{2v_{\frac{1}{2}l} - (v_0 + v_l)} \cdot \frac{l}{4} = \tfrac{1}{4}l + (v_{\frac{1}{2}l} - v_0) \cdot \frac{p}{l} \qquad\qquad (5b).$$

$$V = \frac{\left\{4v_{\frac{1}{2}l} - (v_0 + v_l)\right\}^2 - 4v_0\,v_l}{8\left\{2v_{\frac{1}{2}l} - (v_0 + v_l)\right\}} = v_0 + \frac{Z^2}{p} \qquad\qquad (5c).$$

[The whole of the data available (v_0, $v_{\frac{1}{2}}$, v_1) being required for the determination of the parabola, this case of course affords no evidence whatever for or against the approximation of the velocity-curve to a parabola].

3b. *General Case.*—The Case of more than 3 velocity-measurements available is by far the most common (and most useful) case: in the present Experiments all the curves but one (Ser. 41) fall under this case. Any three of the data taken together would suffice to completely determine a velocity-parabola. But inasmuch as the data are all more or less affected by observation-errors, the parabolæ determined by each group of three data will usually be all different: and it becomes necessary to select a curve which shall agree approximately with all the observations.

There are two ways of doing this—

i. *By trial and error.* An easy, but unsatisfactory way.

ii. *By the Method of Least Squares.* A laborious process, but satisfactory.

3c. METHOD i. *By trial and error.*—This Method is an easy one. The observation-curves are first plotted to scale, and approximate values are assigned (by inspection) for the maximum velocity (V), and depth of line thereof (Z). With these "trial values" several "trial values" of the parameter (p) are to be calculated from the equation (2),—

$$p = \frac{(z-Z)^2}{V-v} \quad \text{.........................(2, } bis\text{)},$$

by substituting the known velocity-measurements (v) at several different depths (z).

[The various data (*i.e.*, known values of v) are by no means equally good for this purpose: the best data are those which—provided they be sufficiently reliable—are the furthest removed from the line of maximum velocity; as otherwise in case of lines near the line of maximum velocity, a small error in the velocity-data will affect the resulting value of p enormously; unfortunately for the successful use of this mode, the velocity-measurements near the bed, which would otherwise be the best for the purpose, are usually the most largely affected by observation-errors (Ch. IX, 9), at any rate when obtained by the Double-Float].

It will commonly happen that on a first attempt the "trial values" of p so found will be *extremely* discordant: they must then be re-calculated with fresh "trial values" of V, Z; if the resulting "trial values" of p are still very discordant, the process must be repeated, changing the trial values of V, Z each time until the resulting "trial values" of p agree *tolerably closely*. Very small changes in the "trial values" of V will suffice, for in consequence of the smallness of the differences ($V-v$) which form the denominator of the expression (2, *bis*) for p, a very small change in V affects the results largely. A close agreement in the resulting values of p cannot be expected, in consequence of the fact already noticed (Art. 1) that the observation-errors are of the same order as the second differences $\Delta^2 v$ of the velocities upon which alone p depends.

When this tolerable agreement (as above) has been obtained, a rough mean of these last trial values of p, and the trial values of V, Z used in obtaining them, may be

taken as provisional final values. It only remains to calculate all the velocities (v) for all the depths z from the formula—

$$v = V - \frac{(z - Z)^2}{p}, \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2, ter).$$

If the calculated values (v') agree pretty closely with the original velocity-measurements—as they usually will—the provisional values used may be accepted as the final values of V, Z, p required.

The result obtained is simply,—

"a velocity-parabola agreeing only pretty closely with the observations".

[This method was used for finding the parabolic elements in the 1874-5 Report, Art. 60].

3d. *Method* i *unsatisfactory*.—The unsatisfactory part of this process is that there is no evidence whatever that the result obtained is the closest that could be got, that is the best obtainable from the data. If the result aimed was merely to obtain a curve agreeing nearly with the observations—and not to discuss the dependence of Z and p on the "External Conditions"—*this end will have been attained* sufficiently for all practical purposes.

But inasmuch as the observation-errors (*i.e.*, errors in the Average values) are of the same order as the second differences of the velocities on which Z, p depend, the *probability is very great that the values of* Z, p *thus obtained are downright bad approximations*. Moderate changes in Z, and large changes in p, will affect the final test (the comparison of the calculated velocities with the original velocity-measurements) in a quite trifling degree, so that the apparent close agreement in this final test (which is in fact certain to result) is *no proof whatever that good values of* Z, p *have been found*.

Thus, if it be proposed to discuss the dependence of Z and p on the "External Conditions", it is absolutely essential to any useful discussion to use a process which shall give values of Z, p which are either really good approximations, or at any rate the best obtainable from the (necessarily imperfect) data.

The only merit of this process of "trial and error" is its *comparative* ease.

[The velocity-parabolæ of the Mississippi Report were *all* obtained in this way, (*see* Report, pp. 233, 234,) as were also those of the Bazin Experiments (p. 228, *et seq.*), and it is a most serious drawback to the value of their conclusions—not as to the fair agreement of the parabolæ with the observation-curves, (which is of course pretty close,) but—as to the dependence of Z and p on the "External Conditions". The probability of large error in the latter is very great. Indeed it is said by one of the critics of the Mississippi Results (Dr. Hagen, Wasserbaukunst, Part II, Die Ströme, p. 291, Vol. I) that "it is 30 billions to 1 against the formula truly expressing any single vertical curve"].

3e. METHOD ii. *By Method of Least Squares*.—The advantage of this Method is that the parabola, and therefore also the parabolic elements Z, V, p which it gives, are *most probably* (*though not certainly*) *the best which can be obtained from the data*, and that it further gives the "probable error" of each of the quantities Z, V, p, thus *affording a measure of confidence* in the Results. The disadvantage of this Method is the labor of its application. To establish any degree of confidence, however, in the resulting values of Z, p however, it seems essential to employ it: and it *was accordingly adopted in* preparing the Results in this Work.

4. Weights of velocity-measurements.—It has been explained that the data (the average velocity-measurements) are by no means equally reliable at all depths, as the Instrument from which they were obtained (the Double-Float) decreases in efficiency (Ch. IX, 9) with increase of depth of the Sub-Float. It is desirable, therefore, to make allowance for this in such a way that the results shall depend on the observations in some way *in proportion to their precision.*

This was done by assigning weights to the observations decreasing with the depth below the surface, viz., by assigning the weight 1 to the observation at the greatest depth, the weight 2 to that next above, 3 to the next higher, and so on, so that if n be the (number of feet of depth in the) greatest depth at which an observation was made, the weights (g_z) of the observed data at the several depths (z) are taken as—

Depths (z) below surface, (in feet),	0,	1,	2,	3,	.... z		$n-2$,	$n-1$,	n.
Weights (g_z) of vely.-measurements,	$n+1$,	n,	$n-1$,	$n-2$,	...$(n+1-z)$...		3,	2,	1.

5. Most Probable Parabola.—It is proposed to style the particular curve and the elements Z, V, p of the same resulting from the application of the "Method of Least Squares"—for shortness' sake—by the names MOST PROBABLE PARABOLA, MOST PROBABLE PARABOLIC ELEMENTS.

The labor of this Method is always very great: but in the present case, from the velocity-measurements having been always effected at the depths 0, 1′, 2′, 3′, &c., (always simple integer numbers,) the labor is far less than it would otherwise be, and a great deal of that labor may be done once for all, and presented in a form *immediately available for future similar cases.* It seems, therefore, well worth while publishing these results, and an outline of the steps leading up to them.

[This occupies Art. 5a—6a. The Reader who is not interested in the *development* of the formulæ is recommended to omit these Articles, except Art. 5c].

5a. Most probable parabolic elements.—The equation (2) of the velocity-parabola before given, is inconvenient for the application of the Method of Least Squares, in consequence of involving the product $(p\text{V})$ and square (Z^2) of some of the sought quantities. It may, however, by obvious transformations, be written in the form

$$v = \text{A} + \text{B}z + \text{C}z^2, \qquad\qquad (6),$$

which is convenient for the purpose, in consequence of the new quantities sought (viz., A, B, C) being involved only in the first degree. It is required then to determine the *most probable values** of these new parabolic elements A, B, C by the

* The whole of the Formulæ and Results of Art. 5a—6a have been checked by Lieut. J. H. C. Harrison, R.E. The numerical work has been further verified by a skilled computer.

Method of Least Squares. These having been so found, those of Z, V, p may be found from them as follows :—

The fundamental equation (6) may, by obvious reductions, be written

$$\left(A - \frac{B^2}{4C}\right) - v = - C\left(z + \frac{B}{2C}\right)^2 \quad\text{.........................(6a)},$$

by comparing which with the primary form (2), it is at once seen that—

$$p = -\frac{1}{C}, \quad\text{.........................(7a)},$$

$$Z = -\frac{B}{2C}, \quad\text{.........................(7b)},$$

$$V = A - \frac{B^2}{4C}, \text{ or} = A + \tfrac{1}{2}BZ, \quad\text{.........................(7c)},$$

from which results p, Z, V may be calculated, when A, B, C have been found.

5b. *Calculation of* A, B, C.—The notation and terminology of this and succeeding Articles is the same as that used in Mansfield Merriman's "Elements of the Method of Least Squares", Art. 33 to 37, to which reference should be made for the rationale of the process.

Attaching for distinctness the subscript z to the symbol v to indicate the velocity (thus v_z) at depth z, the equation of the velocity-parabola is

$$v_z = A + Bz + Cz^2, \quad\text{.........................(6, bis)},$$

in which the most probable values of A, B, C are required.

The data are the $(n+1)$ known velocity-measurements $v_0, v_1, v_2 \ldots\ldots v_n$ at the several depths 0, 1, 2, ... n feet, with weights $(n+1), n, (n-1) \ldots\ldots 1$ respectively, (Art. 4.)

Substituting these values of v_z and z into the fundamental equation (6)

$$\left.\begin{array}{l} A + 0 . B + 0^2 . C = v_0 \\ A + 1 . B + 1^2 . C = v_1 \\ A + 2 . B + 2^2 . C = v_2 \\ \ldots + \ldots + \ldots = \ldots \\ \ldots + \ldots + \ldots = \ldots \\ A + n . B + n^2 . C = v_n \end{array}\right\} \text{are the } (n+1) \text{ "observation equations" of } \textit{different weights}, \quad\text{.........(8)}.$$

Multiplying each equation by the square root of the weight of the velocity-measurement (v_z) in question—

$$\left.\left.\begin{array}{rrrr} \sqrt{n+1}.A+ & \sqrt{n+1}.0.B+ & \sqrt{n+1}.0^2.C= & \sqrt{n+1}.v_0 \\ \sqrt{n}.A+ & \sqrt{n}.1.B+ & \sqrt{n}.1^2.C= & \sqrt{n}.v_1 \\ \sqrt{n-1}.A+ & \sqrt{n-1}.2.B+ & \sqrt{n-1}.2^2.C= & \sqrt{n-1}.v_2 \\ \ldots\ + & \ldots\ \ldots\ + & \ldots\ \ldots\ = & \ldots \\ \sqrt{n+1-z}.A+ & \sqrt{n+1-z}.z.B+ & \sqrt{n+1-z}.z^2.C= & \sqrt{n+1-z}.v_z \\ \ldots\ \ldots\ + & \ldots\ \ldots\ + & \ldots\ \ldots\ = & \ldots \\ \sqrt{2}.A+ & \sqrt{2}.(n-1).B+ & \sqrt{2}.(n-1)^2.C= & \sqrt{2}.v_{n-1} \\ \sqrt{1}.A+ & \sqrt{1}.n.B+ & \sqrt{1}.n^2.C= & \sqrt{1}.v_n \end{array}\right\} \begin{array}{l}\text{are the} \\ (n+1) \\ \text{" reduc-} \\ \text{ed equa-} \\ \text{tions"} \\ \text{of } \textit{equal} \\ \textit{weight,}\end{array}\right\} (8a).$$

It is now convenient to introduce three new symbols L, M, N as abbreviations for the following quantities in which the symbol Σ_0^n indicates a summation with respect to z from $z = 0$ to $z = n$, viz.—

$L = \Sigma_0^n(\overline{n+1-s}.v_s)$, $M = \Sigma_0^n(\overline{n+1-s}.sv_s)$, $N = \Sigma_0^n(\overline{n+1-s}.s^2v_s)$,...(9).

To find the "normal equations", proceed as follows :—

"Multiply each equation by the coefft. of A therein, (*i.e.*, the sth by $\sqrt{n+1-s}$), and add",..(A).

"Multiply each equation by the coefft. of B therein,(*i.e.*, the sth by $\sqrt{n+1-s}.s$), and add",..(B).

"Multiply each equation by the coefft. of C therein,(*i.e.*, the sth by $\sqrt{n+1-s}.s^2$), and add",..(C).

The results are the required "normal equations", viz.—

$$\Sigma_0^n(\overline{n+1-s}).A + \Sigma_0^n(\overline{n+1-s}.s).B + \Sigma_0^n(\overline{n+1-s}.s^2).C = L, \text{(10L)},$$

$$\Sigma_0^n(\overline{n+1-s}.s).A + \Sigma_0^n(\overline{n+1-s}.s^2).B + \Sigma_0^n(\overline{n+1-s}.s^3).C = M, \text{(10M)},$$

$$\Sigma_0^n(\overline{n+1-s}.s^2).A + \Sigma_0^n(\overline{n+1-s}.s^3).B + \Sigma_0^n(\overline{n+1-s}.s^4).C = N, \text{(10N)},$$

from which the values of A, B, C are to be calculated.

It will be observed that the co-efficients of A, B, C depend *only on the numbers* n, s, and not on the observations (v_s) themselves, and also that the co-efficients which multiply v_s in the three expressions L, M, N depend *only on the numbers* n, s, and not on the observations themselves. These quantities may therefore be calculated *once for all*: they are given in Abstr. Tab. 5 for all values of n from $n = 3$ to $n = 10$. [The number $n = 3$ is the lowest for which these formulæ could be required, and the number $n = 10$ is the highest required in the present Experiments].

The middle portion of the Table will facilitate the calculation of the quantities L, M, N (which alone involve the velocity-measurements), and the upper portion will enable the left hand side of the three "normal equations" (10L, M, N) to be written down *by inspection*.

But inasmuch as the co-efficients of A, B, C do not involve the observation-quantities (v), the actual *solution* also of the equations may be effected *once for all*, so as to exhibit the values of A, B, C explicitly in terms of L, M, N.

The solutions must necessarily take the following form :—

$$A = \frac{\lambda_1 L + \mu_1 M + \nu_1 N}{\alpha};\ B = \frac{\lambda_2 L + \mu_2 M + \nu_2 N}{\beta};\ C = \frac{\lambda_3 L + \mu_3 M + \nu_3 N}{\gamma}, \text{(11)},$$

where the new symbols $\lambda_1, \mu_1, \nu_1, \alpha$; $\lambda_2, \mu_2, \nu_2, \beta$; $\lambda_3, \mu_3, \nu_3, \gamma$ are simply numerical co-efficients depending solely on the known co-efficients of A, B, C given in Tab. 5, *i.e.*, solely on the value of n, and may therefore be worked out once for all. Their values are given in the lower portion of Tab. 5, together with some additional quantities explained below.

The most probable values of A, B, C are given then by the Results (11) with the help of the Results in the lower portion of Tab. 5.

[To determine the most probable parabola in any future case then in which n does not exceed 10, is thus reduced to *simple arithmetical* work, viz.—

STEP I. Calculate L, M, N from Eq. (9) with help of middle portion of Tab. 5.

STEP II. Calculate A, B, C from Eq. (11), with help of lower portion of Tab. 5].

These having been found, it remains only to calculate Z, V, p from Results (7*a*, *b*, *c*), and the Most Probable Parabola is completely determined].

5c. *Application.*—The parabolic elements (A, B, C) having been calculated as above, the next Step is to calculate the velocity-ordinates (v_z') thereof from the fundamental formula (6),

$$v_z' = A + Bz + Cz^2, \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(12),$$

for all the depths ($z = 0$ to $z = n$) of velocity-measurement. It is convenient to use the following notation :—

v_z' = calculated velocity-ordinate at depth z.

v_z = actual velocity-measurement at depth z.

It remains to write down all the DISCREPANCIES between the measured and calculated values, *i.e.*, the differences ($v_z - v_z'$). If these Discrepancies, technically termed Residuals, be *all small quantities*, this will be fair evidence that the Observation-curve is really approximately a parabola, and the smallness of these Discrepancies will be a measure of the degree of approximation to a parabola.

6. Probable Errors.—The Method of Least Squares gives also the means of calculating the "probable errors" of the Results. The labor of the calculation is considerable, but as it is the *only means of giving a measure of confidence in the Results*, it is well worth undertaking. And here again a good deal of the preliminary work can be done once for all. The following notation of Merriman's "Method of Least Squares" will be used :—

g_z = weight of velocity-measurement v_z, (given in Art. 4.)

G_a, G_b, G_c = weights of A, B, C.

r_z = probable error of calculated velocity-ordinate (v_z').

R_a, R_b, R_c = probable errors of A, B, C.

R_z, R_v, R_p = probable errors of Z, V, p.

Then (*see* Least Squares, Art. 38), with notation of Art. 4, 5*b*, *c*—

$$G_a = \frac{\alpha}{\lambda_1}, \quad G_b = \frac{\beta}{\mu_2}, \quad G_c = \frac{\gamma}{\nu_3} \ldots\ldots\ldots\ldots\ldots\ldots(13).$$

$$r_1 = \cdot 6745 \sqrt{\frac{\Sigma_0^n g_z (v_z - v_z')^2}{n-2}} \ldots\ldots\ldots\ldots\ldots\ldots(14a).$$

$$r_z = r_1 \div \sqrt{g_z}, \ldots\ldots\ldots\ldots\ldots\ldots 14b).$$

$$R_a = r_1 \div \sqrt{G_a}, \quad R_b = r_1 \div \sqrt{G_b}, \quad R_c = r_1 \div \sqrt{G_c}, \ldots\ldots (14c).$$

Now the values of G_a, G_b, G_c and also their square roots obviously admit of computation once for all: they will be found in the lower portion of Tab. 5. Hence the probable errors (r_z) of the several calculated velocity-ordinates (v_z'), and also those (R_a, R_b, R_c) of the parabolic elements A, B, C may be at once found from Results (14*b*, *c*), when the single quantity r_1 has been calculated, by simply dividing r_1 by the square roots of the several weights (g_z and G_a, G_b, G_c) which are all known.

The quantity r_1 is the only one requiring special calculation in each particular case. Calculate the value of the quantity—

$$\Sigma_0^n g_z (v_z - v_z')^2 = \left\{ \begin{array}{l} \text{Sum of products of squares of Residuals } (v_z - v_z') \text{ multi-} \\ \text{plied by their respective weights } (g_z)\text{, from surface } (z = 0) \\ \text{to bed } (z = n). \end{array} \right.$$

The calculation of the Residuals ($v_z - v_z'$) has been already explained in Art. 5c. The value of r_1 is then at once found from Result (14*a*).

6a. Probable Errors of Z, V, *p*.—The quantities Z, V, p are given as functions of A, B, C by the Results (7*a*, *b*, *c*) of Art. 5a. It is shown in Art. 22 of

Appendix to Vol. II of Chauvenet's Practical Astronomy, that if R be the probable error sought of a quantity X, which is given as a function of the quantities x', x'', x''', whose probable errors R', R'', R''' are known, then

$$R^2 = \left(\frac{dX}{dx'}\right)^2 . R'^2 + \left(\frac{dX}{dx''}\right)^2 . R''^2 + \left(\frac{dX}{dx'''}\right)^2 . R'''^2, \qquad \text{(15).}$$

Applying this Rule, and differentiating the expressions for Z, V, p given in Art. 5a with respect to A, B, C—

To find R_p, we have $\frac{dp}{dC} = \frac{1}{C^2} = p^2$.

$$\therefore R_p = \pm \frac{1}{C^2} \cdot R_c = \pm p^2 . R_c \qquad \text{(16a).}$$

To find R_z, we have $\frac{dZ}{dB} = \frac{-1}{2C}, \ \frac{dZ}{dC} = \frac{B}{2C^2}$

$$\therefore R_z = \pm \frac{1}{2C} \cdot \sqrt{R_b^2 + \frac{B^2}{C^2} \cdot R_c^2} = \mp p . \sqrt{\tfrac{1}{4} R_b^2 + Z^2 . R_c^2} \qquad \text{(16b).}$$

To find R_v, we have $\frac{dV}{dA} = 1, \ \frac{dV}{dB} = - \frac{B}{2C} = Z, \ \frac{dV}{dC} = \frac{1}{4} \cdot \frac{B^2}{C^2} = Z^2$.

$$\therefore R_v = \pm \sqrt{R_a^2 + \frac{1}{4} \cdot \frac{B^2}{C^2} \left(R_b^2 + \frac{1}{4} \frac{B^2}{C^2} \cdot R_c^2\right)} = \pm \sqrt{R_a^2 + Z^2 . (R_b^2 + Z^2 . R_c^2)} \qquad \text{(16c).}$$

The probable errors of p, Z, V may then be calculated by the formulæ (16a, b, c), when the probable errors (R_a, R_b, R_c) of A, B, C are known.

7. Present Work. VELOCITY-PARABOLÆ.—The formulæ just developed have been applied to 45 (out of the 46) of the Average Velocity-Curves of these Experiments (*i. e.*, to all but one, Pl. XIII, Ser. 10, which was considered too *irregular). The resulting Velocity-Parabolæ are accordingly most probably the *best† obtainable from the data.* The RESULTS are exhibited both in the Tables and Diagrams: thus—

See foot of each SERIES in Tab. VII—XXVIII.

Line v shows the Average Velocities (v_z) of observation.

Line v' shows the Calculated Velocity-ordinates (v_z').

Line Δ shows the Discrepancies ($v_z - v_z'$).

Both values (v_z, v_z') are figured on the Diagrams (Pl. XII—XVIII), also—

The Observation-Curve is shown by a clear black line.

The Velocity-Parabola is shown by a dotted line.

The position of the max. velocity line is defined by arrows indicating its two ends.

From the smallness of the Discrepancies in the numerous (45) Curves of these Experiments, as shown in both the Tables and Diagrams—

Tables—by the small values of ($v_z - v_z'$),

Diagrams—by the pretty close coincidence of the two Curves,

and taking into consideration the similar close accordance in the Mississippi and Darcy-Bazin Results, the Conclusion may be fairly drawn, that it is a *general law of fluid motion* (in open channels in long uniform reaches) that—

* The irregularity of this Curve probably results from the poorness of the approximation to Average Velocities, this Series (No. 10) containing only 2 Sets.

† The velocity-parabolæ Ser. 1, 2, 4, 12, Pl. XII, XIII of this Work—computed as above—all lie closer to the Observation-Curves than those obtained by Method 1 for the same Observation-Curves in the 1874-75 Report, (*see* Ser. 14B, 13B, 10B, 12B and Pl. VIII of that Work).

"The Average Vertical Velocity-Curve approximates in general closely to a common parabola with a horizontal axis", ...(17), except of course on verticals which are exceptionally situate, *e.g.*, close to a margin of irregular figure (*e.g.*, Ser. 44—46, Pl. XVIII). The degree of the approximation (in the present Experiments) is also obviously pretty close in general. It will be noticed that the Discrepancies (in both the Tables and Diagrams) are generally much the largest near the bed: this is of course due to the *small weights assigned* (Art. 4) to the velocity-measurements near the bed, the effect of which is that the resulting Curve necessarily lies closest to the upper part of the Observation-Curve.

[The general close agreement cannot be taken as any proof of a close approximation to the values of Z and p, which define the position and size of the parabola].

7a. *Exceptional Cases.*—The three Curves, Ser. 44, 45, 46, Pl. XVIII, upon the exceptionally situate vertical shown in the Cross-section, could not of course be expected to resemble parabolæ. The water flowing past this vertical must obviously meet with very different lateral resistance from the upper and lower parts of the flight of steps close by; which are at very different distances from the vertical in question.

The OBSERVATION-CURVES show by their shape, that—

"The Resistance increases rapidly and pretty regularly from the Surface downwards as the steps approach closer to the vertical in question: throughout this region the Curves are pretty regular", ...(18*a*).

"The Resistance does not increase markedly below the level of the lowest step, where the vertical in question is everywhere very close to the 4′ drop-wall: and these is a marked discontinuity in the curves at the level (of the lowest step) at which the change of figure of the Resisting Border occurs", ... (18*b*).

The Velocity-Parabolæ on this vertical have accordingly been calculated *only from the velocity-measurements above the lowest step*, and may be seen to agree pretty closely with the Observation-Curve above that level: it was not thought worth while prolonging them below that level.

7b. Present Work, PARABOLIC ELEMENTS, (Abstr. Tab. 3, 4).—The PARABOLIC ELEMENTS (A, B, C, Z, V, p) of the MOST PROBABLE PARABOLÆ having been calculated for the 45 selected Average Vertical Velocity-Curves by the formulæ above developed, are *most probably the best obtainable from the data.* The Probable Errors (R_a, R_b, R_c, R_z, R_v, R_p) have also been calculated from the formulæ given: these show the measure of the confidence that can be placed in the approximation obtained to the parabolic elements.

The values of Z, V, p (which alone *possess any geometric interest) are shown in Col. 11 of Abstr. Tab. 3, 4, with their probable errors R_z, R_v, R_p printed (in old brevier type) beneath them, thus $\begin{matrix} Z & V & p \\ R_z & R_v & R_p \end{matrix}$

It will be seen that—

1°. "The probable error (R_v) of V is *generally small*, (seldom exceeding a few hundredths of a foot per second,)" .... (19*a*).

* It has not been thought worth while to publish the values of A, B, C which are merely auxiliary quantities (of no geometric interest) introduced to facilitate the application of the Method of Least Squares.

[In two cases, Ser. 29, 30, it amounts to ·12, ·15 respectively: these Series were very difficult of execution, from the closeness of the vertical of Experiment to the bank, (Pl. XVI)].

2°. "The probable error (R_z) of Z is *frequently by no means small*",.....(19*b*).

[The quantity R_z must not of course be compared with Z itself, but with the full depth (H)].

3°. "The probable error (R_p) of p is *frequently very large*", ...(19*c*).

The large size of the probable errors of Z and p is of course due to the flatness of the Curves, as explained in Art. 1; and points to the Conclusion that—

"The data (velocity-measurements) do not admit of the determination of the position and size of the Velocity-Parabola with any closeness", ...(20).

[The position of the Curve is defined by the value of Z, which fixes the depth of axis and the size of the Curve by the value of p].

[The labor involved in this calculation was very great. The computation occupied three computers,—viz., the author himself (by whom most of the original work was done) and his two Assistants, even with the aid of 3 Crelle's Multiplication Tables and two Arithmometers—*continuously for upwards of a month*].

8. Vertical Curves, Abstract Results, (Tab. 3, 4, & Pl. XXI, XXII).—The Abstract Tables 3, 4 show the principal velocities* ($V, v_0, v_{\frac{1}{2}H}, U, u_m, u, v_H$), the Surface-Fall ($F_1, F_2, F_3$) in Upper, Middle, and Lower Sub-Reach, the Average Wind, and the parabolic elements (V, Z, p, and $\zeta = Z \div H$), for each of the 46 Vertical Velocity-Curves (with a few omissions only, *e.g.*, Ser. 2 & 10). A thorough examination of these Tables will show that all the primary quantities (velocities and parabolic elements) vary *in some very complex manner* partly with the depth, partly with the Surface-Fall, &c. This examination is, however, much more easily conducted with the aid of Diagrams. Pl. XXI, XXII have been constructed to facilitate this for the case of the 28 Curves (Ser. 1—28) on Central Verticals, viz.—

Pl. XXI, Ser. 1—20, of Soláni Twin† Aqueducts.

Pl. XXII, Ser. 21—28, of Soláni Embankment.

For each Vertical Velocity-Curve the principal velocities* (V, v_0, v_H, U, u), the Surface-Falls (F_1, F_2, F_3) in the Upper, Middle and Lower Sub-Reach, and the parabolic elements (V, Z, p, ζ), have all been plotted (with a few omissions only, *e.g.*, Ser. 2 & 10) as *ordinates* to the Depths on Vertical (H) taken as abscissæ. The average Wind for each Curve has been plotted as explained in Ch. V, 21d. Thus all the Results of any one Series are *plotted on one ordinate*.

[The discussion, as far as concerns primary velocities, has already been given in part in Ch. X, 15, and will be continued in Chap. XIV, 11. The discussion below concerns the parabolic elements only].

9. *Parabolic formulæ for p.*—Writing $z = 0$, $z = H$, (and therefore $v = v_0$, $v = v_H$) successively in the fundamental expression, Eq. (2), for p;—

* With exception of V, these are all the experimental values; the value of V alone is that of the most probable parabola: *see* also explanation at head of each Table.

† The similarity of cross-section of the two Soláni Aqueduct Sites is so great, that it seemed advisable to plot their Results together on Pl. XXI. The Results for the two Sites are easily distinguished, by the difference of thick and thin ordinates for the Left and Right Aqueduct respectively.

$$p = \frac{Z^2}{V - v_0}, \text{ or} = \frac{(H - Z)^2}{V - v_H}, \qquad \text{(21a)},$$

$$= \frac{1}{12} \cdot \frac{H^2}{v_{\frac{1}{2}H} - U}, \text{ (this will be shown in Ch. XIV, 9f, (46)), (21b).}$$

In whatever way therefore p may be actually computed, its value depends—for a given depth of water (H)—*only on the difference* $(v_{\frac{1}{2}H} - U)$. Now this difference is usually very small, (*see* Abstr. Tab. 3, 4,) so that a minute error in the determination of *one* of the primary quantities $(v_{\frac{1}{2}H}, U)$ is liable to affect the value of p enormously, a difficulty inherent in the investigation. For this reason none of these expressions will suffice for the original computation of p, without slight alterations in the values of the primary velocity-data, (similar to those explained under Art. 8, *et seq.*, Methods i and ii,) such as will make the whole of them approximate closely to some parabola.

[This is well shown in Abstr. Tab. 7, wherein the value of the quantity $\frac{1}{12} H^2 \div (v_{\frac{1}{2}H} - U)$ calculated from the *unadjusted values* of $v_{\frac{1}{2}H}$, U, *i.e.*, from the Average Velocity-measurements themselves, is tabulated for each of the 27 central vertical velocity-parabolæ (Ser. 1 — 28, omitting Ser. 10) for comparison with the parameter (p) itself. The disagreement will be seen to be extreme. Some of the "unadjusted values" of $(v_{\frac{1}{2}H} - U)$ are indeed negative (Ser. 9, 21)—an impossible condition in a real parabola—thus giving negative (impossible) values to the parameter. In fact the computation ought of course to be done with the "adjusted" (in this case the "most probable") values of $v_{\frac{1}{2}H}$, U].

9a. *Parabolic Formulæ for* Z.—From the equation (4) of the curve,

$$Z = \tfrac{1}{2}s - (v_0 - v) \cdot \frac{p}{2s}, \text{ in general,} \qquad \text{(22).}$$

Writing $s = \frac{1}{\sqrt{3}} H, \frac{1}{2}H, \frac{3}{4}H, H, \left(\text{and therefore } v = v_{\frac{1}{2}H\sqrt{3}}, v_{\frac{1}{2}H}, v_{\frac{3}{4}H}, v_H\right)$ successively in this, there result—

$$Z = \frac{1}{2\sqrt{3}} H - \left(v_0 - v_{\frac{1}{2}H\sqrt{3}}\right) \cdot \frac{\sqrt{3}p}{2H}, \quad \text{or} = \tfrac{1}{4}H - (v_0 - v_{\frac{1}{2}H}) \cdot \frac{p}{H} \qquad \text{(22a),}$$

$$= \tfrac{3}{8}H - (v_0 - v_{\frac{3}{4}H}) \cdot \frac{3p}{4H}, \quad \text{or} = \tfrac{1}{2}H - (v_0 - v_H) \cdot \frac{p}{2H} \qquad \text{(22b).}$$

also, $Z = \frac{1}{3}H - (v_0 - U) \cdot \frac{p}{H}$, (as will appear in Ch. XIV, 9, (16)), ...(22c).

Multiplying (22a) by $\frac{2}{\sqrt{3}}$, and subtracting (22c), there results after reduction

$$Z = \frac{\sqrt{3}}{2 - \sqrt{3}} \cdot \left(v_{\frac{1}{2}H\sqrt{3}} - U\right) \cdot \frac{p}{H} \qquad \text{(22d).}$$

Again, substituting the value of $p = \frac{1}{12} H^2 \div (v_{\frac{1}{2}H} - U)$ in Eq. (22d),

$$Z = \frac{v_{\frac{1}{2}H\sqrt{3}} - U}{v_{\frac{1}{2}H} - U} \cdot \frac{H}{4(2\sqrt{3} - 3)} \qquad \text{(22e),}$$

which is perhaps the simplest expression (not involving the other parabolic elements p, V) that can be found for Z. This shows that the value of Z for a given depth

of water (H) depends—in whatever way it may be actually determined—essentially on the ratio of the two small differences $(v_{\frac{1}{3}H\sqrt{3}} - U) : (v_{\frac{2}{3}H} - U)$, and is therefore difficult to determine accurately, inasmuch as a very small error in any *one* of the velocity-measurements $(v_{\frac{1}{3}H\sqrt{3}}, v_{\frac{2}{3}H}, U)$ will largely affect the value of Z.

[For this reason, neither this expression, nor indeed any of the preceding, suffice for the original computation of Z, without slight alterations in the values of the primary velocity-data, similar to those explained in Art. 8*c*, *d*, *q.v.* In fact the "most probable values" of the velocity-ordinates ought to be used].

From the above it may be inferred that—

$$Z <=> \tfrac{1}{4}H, \text{ when } v_0 >=< v_{\frac{1}{2}H}, \text{..............................} (23a).$$
$$<=> \tfrac{1}{3}H, \text{ when } v_0 >=< v_{\frac{2}{3}H}, \text{ or } U, \text{......................} (23b).$$
$$<=> \tfrac{1}{2}H, \text{ when } v_0 >=< v_H, \text{..............................} (23c).$$
$$\text{is } +, \text{ or } -, \text{ when } v_{\frac{1}{3}H\sqrt{3}} >< U, \text{..............................} (23d).$$

Also since (v_H is usually the *least* of the velocities past each vertical, (Ch. X, 8,) and also $v_0 > U$ in nearly all of the Average Curves on verticals at or near mid-channel (Ser. 1—28 & 35—39, Tab. 3, 4) it follows that—

$$Z < \tfrac{1}{2}H, \text{ in general, for all verticals,..............................} (24a).$$
$$Z < \tfrac{1}{3}H, \text{ in general, on verticals at or near mid-channel,.............} (24b).$$

The above Results (23—24) deduced from the properties of the parabola, are fully borne out by the Experiments, (*see* Abstr. Tab. 3, 4).

10. Parabolic Elements, Variation.—The tracing of the dependence of the two quantities Z and *p* (which determine the position and size of the velocity-parabola) upon the External Conditions, such as depth (H) on the vertical, position of vertical, surface-slope (S), state of wind, &c., has always been a subject of great scientific interest.

But the impossibility of accurate determination (Result (20)) of the quantities Z, *p* themselves must obviously render all attempts to trace the law of their dependence on the External Conditions extremely uncertain.

[Attempts have been made to assign these laws in the Mississippi and Bazin Experts. Reports, and apparently successfully: but the nature of the evidence of the laws assigned is not really good in either case. It amounts in both cases shortly to this—

A few velocity-parabolæ were deduced by Method i, Art. 8c, from a few selected Observation-Curves. The values of Z, *p* so obtained for these selected curves may be said to be independent of any hypothesis as to the dependence of Z, *p* on the External Conditions. From these few values of Z, *p* as data, algebraic expressions were obtained by trial, expressing Z, *p* as functions of the External Conditions H, S, &c.) So far there is *no test* of the truth of these expressions.

The next step was to calculate the values of Z, *p* directly from these assumed formulæ for the whole of the Observation-Curves available, and then with these assumed

values of Z, p to calculate the velocity-ordinates (v_z') for every point of velocity-measurement (v_z) in the Observation-Curves.

The ultimate test was to examine the Discrepancies ($v_z - v_z'$). If these were generally small, it seems to have been assumed that this was a sufficient test of the form of the expressions originally assigned for Z, p.

But the fact is this Test is quite inadequate. For, as already noticed (Art. 8d), considerable changes in the value of Z, and large changes in the value of p, will not materially affect the values of the differences ($v_z - v_z'$), which amounts to saying that—

"Almost any algebraic expression which will give the values of Z roughly, and the values of p even very roughly, will very likely stand this Test equally well",...(25),

or in other words, the Test is quite inadequate to the purpose].

The only adequate mode of this research appears to be to calculate the values of all the parabolic elements (Z, V, p) *independently from the velocity-data of every Observation-Curve* available, (or at any rate from a great many Observation-Curves,) so that these values of Z, V, p may be *in each case independent of each other.* An attempt must then be made to find algebraic expressions which shall express Z, V, p as functions of the External Conditions (H, S, &c.), and satisfy nearly *all* these independent values. The delicacy of the investigation is such that the only hopeful way of conducting it appears to be to use the best values of Z, V, p obtainable from the data, *i.e.*, the Most Probable Values.

[It was chiefly from a conviction of this that the great labor of application of the Method of Least Squares was undertaken for the present Results].

The *cause of the depression* of the maximum velocity line has given rise to so much discussion, that it seems worth while devoting a separate chapter (Ch. XI) to it: the variation of the maximum velocity (V) is not worth* separate discussion: the variation of the parameter (p) alone will be taken up in this Chapter.

11. Parameter-variation.—The expressions proposed in the Mississippi and Bazin Experiments Works will be discussed first; these expressions and various modifications thereof will then be tested by application to the present Experiments. This occupies the rest of this Chapter.

11a. *Mississippi Report, Parameter* (Report, p. 297).—The Equation of the velocity-parabola is written in the following form, wherein the notation has been changed to suit the present work:—

$$V - v = \sqrt{\beta V}\,\frac{(z - Z)^2}{H^2}, \quad\text{.................................(26a)},$$

* The case being sufficiently met (Ch. X, 15, & XIV, 11) in the discussion of variation of Mean Velocity past a Vertical.

wherein $\mathbf{V}$ = mean velocity through whole cross-section,

$$\beta = \frac{1{\cdot}69}{\sqrt{\mathrm{H} + 1{\cdot}5}}, \text{ or} = {\cdot}1856 \text{ when } \mathrm{H} > 80 \text{ feet}, \ldots\ldots\ldots\ldots (26b),$$

Comparing this with the form (Eq. (2), Art. 2) used in this Work, it is seen that the expression proposed for the parameter is—

$$p = \frac{\mathrm{H}^2}{\sqrt{\beta \mathrm{V}}}, \text{ or} = \frac{\sqrt[4]{\mathrm{H} + 1{\cdot}5}}{1{\cdot}3} \cdot \frac{\mathrm{H}^2}{\sqrt{\mathrm{V}}}, \ldots\ldots\ldots\ldots (26c).$$

This Result depends really on the following evidence (Miss. Report, pp. 248—251). The general equation [Eq. (26a) above] was "applied to all the original curves of observation, as first deduced by combining all observations where the depth was the same", with an assumed constant value of β ($\beta = {\cdot}1856$); and afterwards to three additional curves (one original, and two extracted from Boileau's Experiments on wooden canals) with values of β specially calculated to suit them (pp. 251—258). The principal data of these 14 curves are shown in the Table below.

	Step I. Eleven Curves.											Step II. Three Curves.		
	High Water.			Low Water.			Medium Stage.							
	Carrollton.			Carrollton and Baton-Rouge.			Columbus.	Vicksburg.		Bayou Plaquemine.	Bayou La Fourche.	Chesapeake & Ohio Canal.	Wooden Canals (Boileau).	
Reference to Page of Mississippi Report,	248			249			249	249		250	250	253	250, 251	
Total depth (H),..	110'	70'	55'	100'	80'	60'	65'	75'	55'	27'	27'	7'·1	1'·09	0'·62
Number (n) of velocity ordinates,..	7	5	5	15	13	10	6	5	6	5	5	12	15	13
Greatest velocity,.	4·35	3·70	2·90	1·97	2·42	2·39	4·19	7·54	4·58	6·52	3·25	2·52	2·86	2·02
Range of velocities,	·40	·21	·13	·33	·40	·33	·21	·72	·56	·50	·10	·55	·92	·48
Value of β used,..	·1856											·58	1·10	1·07

These curves certainly comprise an immense range of "External Conditions", both of depth (H varies from 0'·62 to 110'), and of velocity (the greatest velocity-measurement varies from 1'·97 to 7'·54 per second). Several of the first eleven curves are, however, not well suited to the purpose from their excessive flatness, which involves in all probability a very poor approximation to p.

[The flatness of these curves is so great—as may be seen by comparing the differences between the greatest and least velocity-measurements (given in last line but one of Table above) which are seen to be very small compared with the depths which are all very large—that if plotted to same horizontal and vertical scales—keeping to the units (feet and seconds) given—the curvature would be quite inappreciable to the eye].

The evidence as to the closeness of the approximation to the values of β (and therefore also of p) is really solely of the kind described in Art. 10, viz.—

With "trial values" of Z and V assumed by inspection of the plotted figure of the observation-curve, and with the assumed value of β (*see* Table above), and with values of V (independently obtained), the velocity-ordinates (v_z') were calculated from the general equation (26*a*) for all the depths (z) of velocity-measurement for comparison with the velocity-measurements (v_z), and the differences or Discrepancies ($v_z - v_z'$) taken out. These Discrepancies ($v_z - v_z'$) are shown (*see* the Tables on pp. 248 to 251 of Miss. Report) to be all very small quantities for all the fourteen curves, and reliance is apparently (p. 250, *ib.*) placed on the smallness of these quantities as proof of the "truth of the formula" itself. But as explained in Art. 8*d* this is good evidence only of the general approximation of the observation-curves to a common parabola, and not of the goodness of the approximation to the values of β or p, because a large variation in p would only slightly affect the small discrepancies ($v_z - v_z'$).

Moreover, it will be seen that (although there were fourteen sets of data available) only four independent values of β were obtained; so that the somewhat complex expression (26*b*) for β, and therefore also that for p, rests on only four data, an altogether insufficient number.

11b. *Bazin Expts., Parameter.*—In the original work (Bazin Expts. 1865) the parabolic form of Vertical Velocity-Curve was proposed only for the case when the maximum velocity is at the surface: and for this case the equation of the velocity-parabola was written (Bazin Expts., p. 228) in the following form, wherein the notation has been changed to suit the present work:—

$$V - v = K \cdot \sqrt{HS} \cdot \left(\frac{z}{H}\right)^2, \quad \text{(when } Z = 0\text{)},\ldots\ldots\ldots\ldots(27a),$$

wherein S = surface-slope,

K = constant = 20 for metric measures, or 36·2 for English feet,....(27*b*).

In a later Essay ("Discussion, &c.," p. 313) this was modified to suit the (far more common) case of the maximum velocity being below the Surface into the following form, the notation being changed as before—

$$V - v = K \sqrt{HS} \cdot \left(\frac{z - Z}{H - Z}\right)^2, \ldots\ldots\ldots\ldots(27c).$$

Comparing this with the form Eq. (2) of Art. 2 used in this Work, it is clear that the later expression for the parameter proposed is

$$p = \frac{(H - Z)^2}{K\sqrt{HS}}, \ldots\ldots\ldots\ldots(27d).$$

This result rests on the following evidence, (*see* "Discussion", pp. 313—325). The general equation (27*a*) above is applied to the following curves:—

15 of the small scale Bazin Experiments, (on canals of 6'·5 wide.)

12 of various Experimenters on European Rivers of moderate size.

11 from the Mississippi Report, and 20 from the Irrawaddi Report.

Its application to the 31 curves taken from the Mississippi and Irrawaddi Reports fails utterly, so that this evidence is ***entirely against the truth*** of the expression, (27*d*).

[This failure is ascribed partly to inaccuracy in the Mississippi and Irrawaddi

Experiments due to the use of the Double-Float; and partly (in the case of the former) to the mode of combination (of doubtful legitimacy) of SETS into SERIES used (*see* "Discussion, &c.," p. 347)].

The application is made by a preliminary modification of the general equation (27*c*) to the obviously equivalent form, more convenient for his system of graphic delineation,

$$\frac{V-v}{\sqrt{HS}}\cdot\left(\frac{H-Z}{H}\right)^2 = K\cdot\left(\frac{z-Z}{H}\right)^2 \text{.......................(27}e\text{)}.$$

This may be written in the shorter form—

$$y' = K\,.\,x'^2, \text{...(27}f\text{)},$$

by introducing the abbreviations—

$$y' = \frac{V-v}{\sqrt{HS}}\cdot\left(\frac{H-Z}{H}\right)^2,\quad x' = \frac{z-Z}{H} \text{.......................(27}g\text{)}.$$

The values of the quantities x', y' are then calculated for each curve (from the observations) for all the depths (z) of velocity-measurement; and these values of y' are then plotted as ordinates to the corresponding *values of* x' taken as abscissæ, thereby giving rise to a sort of modified "observation-curve".

The parabola whose equation is $Y = KX^2$ is also plotted *to the same axes*, giving to K the constant value $K = 20$ in metric measures ($= 36{\cdot}2$ for English feet). The observation-curves just plotted are found in the case of the first 27 curves to coincide *tolerably closely* with the parabola $Y = KX^2$, or in other words, the Discrepancies $(y' - Y)$ between the ordinates of the observation-curve, and parabola are all tolerably small quantities. Thus the evidence as to the closeness of the approximation to p is solely of the kind described in Art. 10, and is therefore not good evidence as to the closeness of the approximation to p. And such as it is, it accords only with the 15 Experiments on the small Canals, and the 12 on moderate sized Rivers, but fails with all the 31 cases on the large Rivers.

11c. *New Trial-parameters.*—It can be seen at a glance from Abstr. Tab. 3, 4, which show the numerical values of the parameter (p), for each of the 45 Vertical Velocity-Curves, and also from Pl. XXI, XXII wherein the parameters (p) are shown (for the case of central verticals only) as ordinates to the depth on each vertical taken for abcissæ, (the curve of p being shown by a chain-dotted line) that—

"The value of the parameter (p) increases on the whole rapidly on any one vertical with the depth (H) on that vertical",..(49).

The Tables and Plates also both show—by the irregularity of the parameter-curve—that the parameter depends in an important way on some other elements than the depth alone.

In consequence of the high theoretical interest of this question, a great many attempts were made to find an expression which should fairly represent the parameter, but with very little success. The following expressions were tried, the forms of which are suggested by those proposed in the Mississippi and Bazin Experts. Reports :—

$$\frac{H^2}{U},\ \frac{H^2}{\sqrt{U}},\ \frac{H^2}{\sqrt{\beta U}},\ (H-Z),\ (H-Z)^2,\ \frac{(H-Z)^2}{\sqrt{U}},\ \frac{(H-Z)^2}{U},\ \frac{(H-Z)^2}{\sqrt{HF_1}}$$

[The Mississippi and Bazin expressions are respectively $H^2 \div \sqrt{\beta V}$ and $(H-Z) \div K\sqrt{HS}$: the expressions $H^2 \div \sqrt{\beta U}$ and $(H-Z)^2 \div \sqrt{HF_1}$ are the nearest to these which the data of the present Experiments afford].

Abstract Table 7 shows the values of each of these expressions, and also of p, R_p, Z, H, U, $(v_{\frac{1}{2}H} - U)$, for all the velocity-parabolæ on central vertical. These eight expressions are also shown in Pl. XXIII plotted as ordinates with the *values of p taken as abscissæ* for each Site separately. This mode of plotting has the advantage that it would show at a glance if any of the expressions tried were either equal to p, or proportional to p, (by the tips of the ordinates lying on a straight line passing through the origin,) or even connected with p in any simple manner (by the tips of the ordinates lying at any rate in some fairly flowing curve, wholly convex or wholly concave to the axis of abscissæ).

A glance at the eight curves will show that—

Fig. 1. All the curves are *pretty regular*, and some of them approximate fairly to straight lines through the origin : so that for this Site, it appears that p is roughly proportional to each of the quantities—

$$(H-Z)^2,\ (H-Z)^2 \div \sqrt{U},\ (H-Z)^2 \div U,\ (H-Z)^2 \div \sqrt{HF_1}$$

Fig. 2. Most of the curves are *extremely irregular* : omitting, however, a few ordinates, viz., those corresponding to $p = 48{\cdot}0,\ 79{\cdot}5,\ 89{\cdot}5$, the following—

$$(H-Z)^2,\ (H-Z)^2 \div \sqrt{U},\ (H-Z)^2 \div U,\ (H-Z)^2 \div \sqrt{HF_1}$$

give *tolerably regular* curves.

Fig. 3. All the curves, except those involving $(H-Z)$, are *extremely irregular* : and of these, if the ordinates corresponding to $p = 30{\cdot}5$ be omitted, the following, viz.—

$$(H-Z)^2,\ (H-Z)^2 \div \sqrt{U},\ (H-Z)^2 \div \sqrt{HF_1}$$

give *tolerably regular* curves.

[In making these comparisons, the amount of "probable error" (R_p) in the values of p must of course be borne in mind : by replotting the Diagrams, changing the values of p by amounts within this limit, the regularity of most of the curves could be *greatly increased*. The "probable errors" R_p are given in Tab. 7 to show what great changes are admissible.

It will be seen that most of the curves whose ordinates involve $(H-Z)^2$ are for all three Sites—much more regular than those whose ordinates involve H^2. It cannot however be said that the endeavor to make the value of p depend on $(H-Z)$ (as proposed by Mr. Bazin) instead of on H is really a step in advance, because the formulæ of Art. 9 show that p may be made to depend on $(H-Z)^2$ or H^2 equally well by a suitable adjustment of the denominator. The formulæ of Art. 9, moreover, show that any *true* expression for p must be *of the order*,

$$p = (\text{measure of length})^2 \div (\text{velocity}), \ldots\ldots\ldots\ldots \quad (28a),$$

and since velocity may be expressed as $\sqrt{2gh}$, wherein h is the only variable, the above may also be expressed as—

$$p = (\text{measure of length})^2 \div (2g \times \text{measure of length})^{\frac{1}{2}}, \ldots\ldots\ldots\ldots (28b).$$

No expression not of this form can be expected to be aught but an empirical one, and therefore only suited to a limited range of data.

[The Bazin expression is of form (28b), but the Mississippi expression is not of either of above forms].

It might be supposed that since the expression $\frac{1}{12} H^2 \div (v_{\frac{1}{2}H} - U)$ gives the value

of p *accurately*, (Art. 9,) so that for a given depth of water (H), p is inversely proportional to $(v_{\frac{1}{2}H} - U)$, the labor of the calculations of such quantities as the trial expressions for p above proposed might be avoided by endeavoring to trace the dependence of the quantity $(v_{\frac{1}{2}H} - U)$ on the external conditions.

It would, however, for the reasons explained in Art. 9, be absolutely necessary to use the "adjusted values" of $v_{\frac{1}{2}H}$, U for this purpose. And, in this case, there might perhaps be some advantage in this process.

It does not seem worth while to pursue this investigation any further: sufficient has been done to show the extreme uncertainty of it: it confirms on the whole the previous Conclusion (20) that the data do not admit of the determination of the parameter with sufficient accuracy to render the attempt to trace its dependence on the External Conditions a very hopeful one.

CHAPTER XII.

DEPRESSION OF MAXIMUM VELOCITY.

Preface.—The subject of this Chapter is chiefly of theoretical interest.

1. *Depression of Max. Velocity.*—Perhaps one of the best established experimental results of modern hydraulics is the depression, as a general rule, of the line of Maximum (Average) Velocity below the surface. The *fact* is generally admitted, but there is considerable controversy among hydraulicians as to the *cause.* Two causes are generally proposed—

1°, Resistance of the Air; 2°, Disturbance communicated from the bed and banks.

[Of course the above applies only to Average Velocities. In comparing single velocity-measurements at different points on the same vertical, the maximum *might* be found anywhere in consequence of the Unsteady Motion].

2. **Various opinions.**—The Opinions of modern Experimenters and Writers will be here quoted at some length, and the evidence from the present Experiments given afterwards.

2a. *Boileau Experts.*—As to the *cause* of the depression of the maximum velocity line, the Conclusions given are—

P. 308—"That cause cannot be solely the resistance of the layer of air in contact with the fluid surface, acting like the sides of a pipe, for the mobility of this layer of air does not permit of attributing to it a retarding influence so great as that shown by the rapid decrease of velocity in the upper part of the current".

And again, remarking on certain cases in which the depression existed even with a wind blowing down-stream, (p. 313)—

"However the layer of air close to the surface, moving in the same direction with it, could not oppose to its motion aught save a very feeble—or perhaps even zero or negative—resistance. It is then chiefly in the mutual actions which subsist between the fluid particles, and in the oblique or rotatory movements which result under the influence of these forces from the difference of velocity of neighboring particles, that the explanation of the phenomenon of the decrease of velocity close to the surface of a stream is to be sought".

2b. *Mississippi Report*, pp. 286, 287.—The Conclusions given are—

"The observations already detailed prove that even in a perfectly calm day there is a strong resistance to the motion of the water at the surface as well as at the bottom,

and—as will soon be seen—that it is not wholly or even mainly caused by friction against the air. One important cause of this resistance is believed to be the loss of living force, arising from upward currents or transmitted motion occasioned by irregularities at the bottom. This loss is greater at the surface than near it".

* * * * * *

"For all general purposes, it may be assumed that there is a resistance at the surface, of the same order or nature as that which exists at the bottom. As the distance from the loci of these two resistances is increased, their effect, propagated by the cohesion of the different particles of water to each other, is diminished. Where these diminished resistances become equal, the current acquires its maximum velocity".

* * * * * *

"The depth of the axis [*i.e.*, line of maximum velocity] varies in direct proportion to the force of the wind, increasing for up-stream, and diminishing for down-stream breezes, but without producing any effect upon the form of the curve".

* * * * * *

"The axis [*i.e.*, line of maximum velocity] can rarely be at rest; every varying breeze, however gentle, most affect its delicate adjustment, while the stronger pulsations of a high wind must produce an oscillatory movement even greater than that in the tops of the tallest trees".

It will be seen that though it is (at first) advanced that the resistance to the motion near the surface is "not mainly caused by friction against the air", nevertheless (a few lines lower) the position of the maximum velocity line is made to depend *solely on the wind*, as is also clear from the formula given for the depth (Z) of this line (pp. 262, 288, 297) in a vertical of depth (H),—

$$Z = (\cdot 317 + \cdot 06f) \, . \, H,$$

in which the only variable is the force of the wind (f).

26. ***Basin Experiments.***—The Conclusions given are—

See p. 24. "It is known that the surface-velocity of the stream is habitually less than at a certain depth below. The resistance of the air does not suffice to account for this decrease near the surface, for it persists even with a wind from up-stream which should accelerate the upper-layers".

Again, (p. 283). "The law of variation of velocity in a channel of immense width is very simple; but if we examine the case in which the depth is so great compared with the width, that the action of the sides may have sensible effect as far as mid-channel, this distribution appears to vary according to very complex laws.

* * * * As the depth increases *

the maximum velocity is found no longer at the surface, but at a pretty considerable depth below. The resistance of the air to the motion of the upper-layer has certainly only a feeble influence on this phenomenon, admitting however, as we have always done hitherto, that the Experiment be done in calm weather".

Again (p. 225). "In flow in an open channel, on the contrary, considerable disturbance is produced near the surface of the stream. This disturbance which appears to be much greater when the velocities are less, causes the maximum velocity to sink to a great depth".

It would seem that the author considers the disturbed motion near the surface to be the prime cause of the depression of the maximum velocity.

2d. *Connecticut Report* of '78, *p.* 314.—The Conclusion given in the discussion of the vertical curves is—

"During the season when the herein-described current experiments were made at Thompsonville, the winds were generally very light, and the greater part of the observations were but slightly affected by the effect of the wind upon the surface of the water. The curves were grouped in various ways to determine the amount of wind effect, but the results have not been very satisfactory. It appears from some of the observations that other causes than the effect of the wind raised and lowered the thread of greatest velocity, which the number of observations taken was not sufficient to eliminate. The best result that could be obtained was that the axis of the vertical curve was raised or lowered about $\frac{1}{150}$ of the depth for each mile per hour the wind was blowing down or up-stream ".

2e. *Moseley's Steady Flow*, (Phil. Mag., Vol. XLII, XLIV).—The view given in this Paper is quite different. After showing (Vol. XLII, p. 352) that *pressure decreases with increase of velocity* (in pipes flowing full), and accepting the fact that *velocity decreases from centre to sides* (in open channels), the Conclusion (Vol. XLIV, p. 44) given is—

"As the pressure is everywhere less where the velocity is greater, it is evident that there will be a tendency in the liquid on the surface to flow from the sides of the channel towards the centre, and that thus the velocity of the surface-water at the centre will be diminished, and the water heaped up, drowning, as it were, the point of greatest velocity in the section ".

2f. *Mr. Francis's Opinion*, (Amern. Socy. of Civ. Engrs. Trans., Vol. VII, No. CLX, May '78).—In this Paper the depression of the maximum velocity is attributed* —not at all to the influence of wind, but—to the rising of slack water from the bed to the surface, and to its subsequent *lateral* spread over the surface, whereby the increase of (down-stream) velocity which it must surely acquire whilst rising through strata of quicker (down-stream) motion is partly expended *in lateral motion*, instead of appearing in the form of an increase of (down-stream) velocity.

In the "Discussion" upon this Paper, none of the speakers advanced the subject much; also none of them attribute *much* influence to the wind.

2g. *Professor Jas. Thomson's Opinion*, (Royal Socy. Procgs., Vol. XXVIII, No. 191 of Decr. '78, Paper I)—

In this Paper Prof. Jas. Thomson gives in effect* much the same explanation as that of Mr. Francis (*supra*).

3. New Evidence.—The Abstract Tables (3, 4) and Diagrams (Pl. XXI, XXII) enable the effect of the following External Conditions on the position of the maximum velocity line to be traced, viz.—

1°. Depth of water. 2°. Surface-Fall, or Velocity. 3°. Wind.

3a. DEPTH, *Effect of*.—A cursory examination of the two curves (Pl. XXI, XXII) showing the—

Actual Depression (Z) of max. velocy. line,

Proportionate Depression ($\zeta = Z \div H$) of ditto,

* Quotations both short and to the point cannot be readily made from this Paper.

for all the 27 velocity-parabolæ on central verticals, (Ser. 1—9, 11—28) will show at once that these curves are extremely irregular, their variation apparently in no way following that of the depth (H) on the vertical: *e.g.*, the highest values of H, (10·89 in Pl. XXII; 9·94, 9·46, 9·41 in Pl. XXI) carry with them both *the highest and the lowest* values of Z and ζ. This points to the Conclusion—

"Neither the actual nor proportionate depression (Z, ζ) of the max. velocy. line depend much on the depth of water (H)", ... (1).

3b. Velocity and Surface-Fall, *Effect of.*—A detailed comparison of the lines of Velocity (V, v_0, U, &c.,) and of Surface-Fall (F_1, F_2, F_3) with those of Z and ζ in Pl. XXI, XXII, will show that for the case of Central Verticals there is a considerable degree of correspondence in the variations of these quantities at the Solání Aqueduct Sites, and to a lesser degree in the Solání Embankment Sites, to the extent that a rise or fall in some one of the former lines is usually *accompanied by a rise or fall* in the lines of Z and ζ, especially of ζ. The same may also be seen in Abstr. Tab. 6.

[This Table is only a re-arrangement of part of Abstr. Tab. 3, 4, arranged in the order of increasing values of the proportionate Depression ($\zeta = Z \div H$). The "probable error" (R_2) of Z will give (when compared with the magnitude of H, not of Z itself) the measure of confidence in the Results. The Serial Numbers enable reference to be made to the Detailed Tables VII—XIX].

Solání Aqueduct Sites. Omitting the case of work done in the Right Aqueduct with the Left Aqueduct closed, (an altogether exceptional state,) it will be seen that—

1°. "The rises and falls in the line of ζ correspond closely with the *rises and falls* in the line of F_1, omitting 4 cases (out of 16), viz., Ser. 3, 14, 15 and either of Nos. 8, 9".

2°. "The rises and falls in the line of ζ correspond closely with the *rises and falls* in the line of U, omitting 2 cases (out of 16), viz., Ser. 2 and 14".

Solání Embankment Site. This Site presents a marked contrast to the former—

1°. "The rises and falls in the line of ζ correspond closely with the *rises and falls* in the line of F_2, (but not of F_1,) omitting 2 cases (out of 8), viz., Ser. 24, 26".

2°. "The rises and falls in the line of ζ correspond closely with the *falls and rises* in the line of U, omitting 2 cases (out of 8), viz., Ser. 24, 28".

With the marked contrast in the manner of correspondence of the line of ζ, with the Surface-Fall and Velocity lines at these Sites, it seems difficult to draw any certain Conclusions.

[This shows the danger of drawing general Conclusions from a small number of data. Had the work at the Solání Aqueduct Sites alone been available, the Conclusion to be drawn would have seemed clear].

Viewing them together, it seems *probable* that—

"The proportionate depth of the max. velocity line on a central vertical increases and decreases with increase and decrease of the Surface-Slope (Surface-Fall near the Site,)" .... (2),

but the evidence is insufficient to enable the quantitative connection to be traced. It seems also (to the Author) probable that the temporary state of the water-surface, *i.e.*, state of *rising*, *being stationary*, or *falling*, may have much to say to this: but

this is a question which cannot be traced at all by Average Results (such as the present ones) in which these states are probably often combined.

3c. WIND, *Effect of*.—Next, a comparison of the curves of Z, ζ with the "Average Wind" plotted at foot of the Diagrams will, with help of Abstr. Tab. 6, enable the effect of the Wind on the position of the max. velocity line to be traced.

It will be seen that the variation of Z, ζ seems to bear hardly any relation to the state of the wind, and that in particular *high values of Z, ζ do not correspond to winds blowing up-stream* (south wind), nor *low values to winds blowing down-stream* (north wind)—as they would upon the Theory advanced by the Mississippi Experimenters--but rather the contrary.

[The Results for different Sites should be compared together—thus the highest up-stream (south) wind gives, *see* Ser. 4, Left Aqueduct, a value of $\zeta = \cdot 194$, which is *lower than any value of* ζ in the Right Aqueduct].

The general Conclusion from this is that—

"The position of the max. velocy.-line is not sensibly affected by the wind", (3).

[It may *perhaps* be that the AVERAGE WINDS in the Series available are not in general high enough (very few exceeding 5 feet per second) to test this point satisfactorily. The mode of combination of Sets into SERIES (Ch. VI, 18) is in fact not well suited to show Wind effects, as opposite winds are in this way frequently combined so as to leave little resultant effect, *see* Ch. V, 21*c*].

4. Wind-effect trifling.—The present Experiments then bear out the Results of most of the previous Experimenters in assigning a comparatively trifling effect to the wind in elevation or depression of the maximum velocity line. That it should be so trifling seems very surprising.

The analogy of the known effects of wind on large bodies of water will perhaps serve to explain this. These are known to take the form chiefly of production of *wave-motion*, not of translation of the surface-water. Were it otherwise, *every high wind* ought to produce a surface-current on a lake and on the ocean, and the water-level should stand markedly higher on the leeward than on the windward shore, which are both contrary to observation *when the wind is only temporary*.

In order that a high wind should prodnce marked effects of translation of the surface-water, it seems essential that the wind should be *sustained for a great length of time*, and in such cases it is well known to produce marked surface-currents, and also elevation of water-level.

[Witness the effects of the trade winds as shown in the equatorial currents; and of the wind which blows steadily up the Red Sea in certain months as shown in raising the water-level at Suez. The acceleration or retardation of the tide which often occurs with a high wind in a narrow channel is rather a case of the wave-effect of wind.

The *time* required for the penetration of the change of velocity of the surface-current caused by wind to any considerable depth appears to be very great. It has

been estimated* that it would take—

1 week for ½ change of surface-velocity to penetrate 3 feet.

1½ days for $\frac{1}{10}$ " " " " "

If this Estimate is in any way approximate, it will serve to explain the slight effect of the wind in elevation or depression of the maximum velocity as observed in canals or rivers; the wind having been probably of short duration in most of the cases].

5. **Air Resistance.**—The Opinions previously quoted against the admission of the Resistance of the Air as one of the chief causes of the depression of the maximum velocity line, appear to be all based on a consideration of the trifling effects of Wind on the position of this line. It seems to have been assumed that if the Resistance of the Air were in any way a sufficient cause of this phenomenon, the effects of Wind ought *à fortiori* to be still more marked.

If the explanations just given of the trifling effect of Wind can be accepted, it seems (to the Author) that the Resistance of the Air must still be admitted as one efficient cause of the depression. It seems that the effects of Wind (in causing translation) are well marked when the Wind is long enough continued to admit of it. This removes the principal difficulty in admitting the reality of the Air Resistance. The ordinary state of the air in all the Experiments on rivers and canals was probably either calm or else wind not sustained in both direction and intensity for a great length of time, so that the average position of the maximum velocity line in (the average of) such Experiments would be usually that due to still air: that is to say, depressed below the surface, (if the air causes any resistance at all.) Now this state (depression) is *admitted to be the ordinary state.*

6. *Flow in Pipes and Open Channels.*—The Diagrams in the "Bazin Experiments" Atlas showing the distribution of velocity through the cross-section of (rectangular) Pipes *flowing full*, and of (rectangular) Open Channels seem, when compared together, to throw some light on this.

It will be seen (Pl. XVIII—XXI of Atlas quoted), that—

PIPES. The Wet-Border had complete symmetry, both geometric and physical, about both axes of the (rectangular) cross-section.

OPEN CHANNELS. The Wet-Border (including the Air-surface in this term) had complete geometric symmetry about both axes of the (rectangular) cross-section, and physical symmetry about the vertical axis, but not about the horizontal axis, (the upper surface being of air, and the lower of rough material.)

The velocity-distribution was found to be very similar in the two cases, (*see* the Plates quoted) *so far as the relations of the Wet-Border were similar*, viz.,—

PIPES. Very symmetrical about both axes of the cross-section.

* by Prof. Zoppritz of Giessen, pub. in Van Nostrand's Maga., Vol. XXI of '79, p. 212.

OPEN CHANNELS. Very symmetrical about the vertical axis of the cross-section: and with the max. velocity line highest on the central vertical, and sinking deeper for verticals nearer the sides.

Thus the two cases agree in symmetry of velocity-distribution about the vertical axis, *i.e.*, just to the extent that the Wet-Borders are similarly related. Next, the symmetry of velocity-distribution about the horizontal axis in the case of the Pipes flowing full seems to be due to the physical symmetry of the top and bottom edges of the Wet-Border. And, the change in that distribution in the case of the Open Channels is just what would seem likely to be due to the substitution of a tolerably smooth upper surface (in place of the rough upper surface of the Pipes) capable of exercising an efficient resistance to the flow, but to a much less degree than that due to the comparatively rough bed.

[In this view the Air-Surface is to be regarded as a part of the Wet-Border, exerting a small but sensible resistance to the flow].

Let the effect of such a smooth upper surface be considered. It would seem probable that—

1°. There would be symmetry of flow about the vertical axis, because there is complete symmetry, both geometric and physical, of the Wet-Border about that axis.

2°. The max. velocity would be nearer the upper than the under surface upon every vertical because of the less resistance at the upper surface;

3°. but would nowhere approach quite close to the upper surface, unless the resistance was zero at some part thereof.

4°. The max. velocity line would be highest on the central vertical, because the resistance of the sides would be least efficient there;

5°. and would sink deeper and deeper on verticals nearer and nearer to the sides, because the resistance of the Border (*i.e.*, sides and top) would become more and more efficient;

6°. and would be deepest (but above mid-depth) close to the vertical sides.

7°. The lines of least velocity would be at the four corners of the cross-section; the resistance being most efficient at the corners, as being due to the close proximity of two resisting surfaces (one vertical, one horizontal) at each corner.

The whole of the above description of what *would seem to be the probable state* of velocity-distribution in a rectangular Pipe with smooth top and rough bed agrees in a general way closely with the Diagrams figured in Pl. XVIII—XXI of the "Bazin Experiments" in the case of small Open Rectangular Canals: and also in a general way (*i.e.*, with some exceptions) with the Results of the present large scale Experiments in the rectangular channel of the Solání Aqueduct (Pl. XIII, XVI, XVII, & XXXA, 4; the Cross-Sections on Pl. XVII, XXXA bear especially on this point).

7. Reviewing then all the evidence, the Conclusion seems fair that—

"The Air-Surface is to be regarded as a portion of the Wet-Border causing a slight but sensible resistance to the flow", ... (4),

also that—

"The Depression of the Max. Velocity Line is due largely to the resistance of the Air, (but not to temporary Wind)", ...(5).

2 D

CHAPTER XIII.

DISCHARGE PAST A VERTICAL.

Preface—The practical bearing of this Chapter is explained in Art. 1, 5. Detailed Formulæ are given and discussed in Art. 2—4b.

1. Discharge past a vertical, (D).—The quantity of water passing a vertical line in one second is evidently a superficial quantity, measureable say in square feet, and will be styled for shortness the DISCHARGE PAST THAT VERTICAL, and will be denoted by the letter D.

It is obviously measured by the Area of the Vertical Velocity-Curve. It is a quantity of not much practical importance in itself; except as being a Step towards the computation of the Total Discharge of a channel.

2. Discharge-Formulæ.—The velocity-measurements made at every foot of depth on the same vertical enable the DISCHARGE PAST THAT VERTICAL to be computed by known approximation formulæ.

2a. APPROXIMATION.—The *best* approximation-formulæ to an Area divided into 1, 2, 3, 4.........n equally broad spaces by equidistant ordinates are given in Works* on the Calculus of Finite Differences: these Rules are equivalent to supposing the boundary to be severally a straight line, parabola, cubic, quartic.........n-ic respectively, and each Rule of higher degree is in general a closer approximation than any combination of Rules of lower degree would be. The labor of application of these (which are the best) formulæ would be excessive, and not worth undertaking, except for the four cases where $n = 1, 2, 3, 6$. In these four cases the formulæ are so simple as to be of easy and rapid application, and they are accordingly given in all the larger Works† on Mensuration, together with their generalizations (which are simply repeated applications) to the case of an Area divided into *any number* (n) of equally broad spaces by equidistant ordinates. These generalizations are equivalent to supposing the boundary curve to be made up of straight lines, arcs of parabolæ, arcs of cubics, or arcs of sextic curves, (and are accurate for such cases, except the last, (Weddle's Rule, which is accurate for a quintic, and is highly approximate for a sextic and even septimic curve,) and each Rule of higher degree is usually a *closer approximation than any combination of Rules of lower degrees.* These Rules are conveniently termed the Trapezoidal, Parabolic (or Simson's), Cubic, and Sextic (or Weddle's).

* *see* Boole's "Calculus of Finite Differences", Chap. III, Art. 7.

† *see* Moore's "Elementary Treatise on Mensuration," Chap. XI.

2b. Formulæ used.—The formulæ actually used in each case in the present Work for the Discharge past the vertical throughout from the surface down to the level (nominally n feet) of the lowest velocity-measurement (v_n), are given in the Table below, and will be found useful for use in like cases hereafter. They are in each case probably about the best* approximation consistent with tolerable facility of application.

The notation used is, in addition to that before given (Ch. X, 9)—

k = width of spacing of ordinates = 1 foot in present Experiments.
n = number of equal spaces.
nk = nominal depth (in feet) of lowest velocity-measurement (v_n).
D_n = Discharge past the vertical from surface to nominal depth n feet.
D = Total Discharge past the vertical from surface to bed.

Nominal Depth. nk	Number of equal spaces. n	Number of velocity-ordinates. $(n+1)$	Rule.	Discharge past the Vertical, (D_n) down to nominal depth of nk feet.	Result No.
1	1	2	Trapezoidal	$\frac{1}{2}(v_0+v_1).k$	(1)
2	2	3	Simson's	$\frac{1}{3}\{(v_0+v_2)+4v_1\}.k$	(2)
3	3	4	Cubic	$\frac{3}{8}\{(v_0+v_3)+3(v_1+v_2)\}.k$	(3)
4	4	5	Simson's	$\frac{1}{3}\{(v_0+v_4)+4(v_1+v_3)+2v_2\}.k$	(4)
5	5	6	Trapezoidal	$\frac{1}{2}\{(v_0+v_5)+2(v_1+v_2+v_3+v_4)\}.k$	(5)
6	6	7	Weddle's	$\frac{3}{10}\{(v_0+v_2+v_4+v_6)+v_3+5(v_1+v_3+v_5)\}.k$	(6)
7	7	8	Weddle's & Trapezoidal	$\frac{3}{10}\{(v_0+v_2+v_4+v_6)+v_3+5(v_1+v_3+v_5)\}.k+\frac{1}{2}(v_6+v_7).k$	(7)
8	8	9	Simson's	$\frac{1}{3}\{(v_0+v_8)+4(v_1+v_3+v_5+v_7)+2(v_2+v_4+v_6)\}.k$.. ..	(8)
9	9	10	Cubic	$\frac{3}{8}\{(v_0+v_9)+2(v_3+v_6)+3(v_1+v_2+v_4+v_5+v_7+v_8)\}.k$	(9)
10	10	11	Simson's	$\frac{1}{3}\{(v_0+v_{10})+4(v_1+v_3+v_5+v_7+v_9)+2(v_2+v_4+v_6+v_8)\}.k$	(10)
11	11	12	Weddle's & Trapezoidal	$\frac{3}{10}\{(v_0+v_2+v_4+v_6+v_8+v_{10})+(v_3+v_6+v_9)$ $+5(v_1+v_3+v_5+v_7+v_9)\}.k+v_{11}.k$	(11)
12	12	13	Weddle's	$\frac{3}{10}\{(v_0+v_2+v_4+v_6+v_8+v_{10}+v_{12})+(v_3+v_6+v_9)$ $+5(v_1+v_3+v_5+v_7+v_9+v_{11})\}.k$ ..	(12)

[It might be supposed that the formulæ above given—judging merely from their apparent length—were troublesome of application, but this is not the case: the coefficients involved are all so simple, and the order in which the odd and even Velocity-ordinates succeed each other is so regular, that with a little method in the way of using them, they are *extremely simple and easy* of application, and can be quickly picked up by the most ordinary computer, at the same time that there is considerable gain in accuracy in their application].

2c. *Correction to bed-level.*—The above Results are the value of the quantity D_n, *i.e.*, of the Discharge down to the nominal depth (n feet) of the lowest velocity-measurement. They require a small correction for the difference between the full

* It is however open to question whether the combinations used when n is not a multiple of 2, 3, 6 are the best. Thus when n = 5, 7, 11, a combination of the Cubic and Simson's Rules would possibly have been better than those used.

depth (H) and above nominal depth (n feet). Two Cases present themselves according as $H <> n$, *see* Ch. X, 13.

i. *Full depth* (H) < *nominal depth* (n feet) *of lowest velocity-measurement.*

The Discharge D_n above computed is obviously *too great;* having been *computed* upon a nominal depth (n feet) > the full depth (H). The only obvious reduction possible is to reduce the computed Discharge (D_n) in the ratio of H to n, thus—

$$\text{Discharge required, } D = \frac{H}{n} \cdot D_n, \qquad (13).$$

ii. *Full depth* (H) > *nominal depth* (n feet) *of lowest velocity-measurement.*

In this case the computed Discharge (D_n) is *too small* by the quantity discharged through the space $(H - n)$ feet between the lowest velocity-measurement (v_n) and the bed. The bed-velocity (v_H) having been already computed (Ch. X, 18), this additional quantity is approximately—

$$\text{Correction to bed-level} = (H - n) \cdot \frac{v_n + v_H}{2}, \qquad (14).$$

and finally,*

$$\text{Discharge required, } D = D_n + (H - n) \cdot \frac{v_n + v_H}{2}, \qquad (15).$$

2d. Simple Rules, *Err in defect.*—For simplicity's sake one or other of the following Rules (apparently simpler than those above given) have sometimes been used in working up other Experiments :—

$$\text{Trapezoidal Rule, } D_n = \tfrac{1}{2}k\,\{v_0 + v_n + 2(v_1 + v_2 + \ldots\ldots - v_{n-2} + v_{n-1})\}\ldots (16).$$

$$\text{Arithmetic Mean Rule, } D = H \times \text{Arithmetic mean of the velocity-measurements}$$

$$= H \times \frac{v_0 + v_1 + v_2 + \ldots\ldots\ldots v_n}{n+1} \qquad (17).$$

[This last Rule seems to have been employed in the Connecticut Experiments, (*see* Report of 1878, pp. 318, 352].

The only merit of these two Rules is their simplicity, but it may be shown that they both *constantly tend to error in defect* in the long run, (though not necessarily of course in single instances.)

All Experiment tends to show that the Average Vertical Velocity-Curve is generally a Curve *wholly convex down-stream,* (*see* Ch. X, 8). Now the Trapezoidal Rule gives only the Area of the inscribed polygon (Pl. XX, 4) formed by joining the extremities of the successive ordinates (v_0, v_1, v_2, ... v_n), and gives therefore a Result obviously too small.

Again, the Trapezoidal Rule gives the Discharge (D_n) only down to the level of the lowest velocity-measurement (v_n); the Arithmetic Mean Rule gives the following Result *down to the same level,* (making $H = nk$).

$$D_n = nk \times \frac{v_0 + v_1 + v_2 + \ldots\ldots\ldots + v_n}{n+1}$$

$$= \left\{(v_0 + v_1 + v_2 + \ldots\ldots + v_n) - \frac{v_0 + v_1 + v_2 + \ldots\ldots\ldots + v_n}{n+1}\right\} \cdot k$$

$$= \left\{\frac{v_0}{2} + (v_1 + v_2 + \ldots\ldots + v_{n-1}) + \frac{v_n}{2}\right\} \cdot k + \left(\frac{v_0 + v_n}{2} - \frac{v_0 + v_1 + v_2 + \ldots\ldots + v_n}{n+1}\right) \cdot k$$

* In the 1874-75 Report the Discharges given are only the values of D_n as above, *i.e.*, computed only down to level of n feet without applying the Bed-correction: this is clearly shown in the headings of the Tables V to IX of that Work.

Now the first of these quantities is the Trapezoidal Rule Result; and it is easy to see that *in a wholly convex curve*, (Pl. XX, 4)—

The Arithc. Mean of the extremes, *m*E in *Fig.* 4, is always < the Arithc. Mean of all the ordinates, *i. e.*, $\frac{v_0 + v_n}{2} < \frac{v_0 + v_1 + v_2 + \dots\dots\dots + v_{n+1}}{n+1}$

Thus, *when compared through the same spaces* (*n* feet), the Arithmetic Mean Rule gives a Result smaller than that of the Trapezoidal Rule, and therefore *à fortiori* too small a Result.

Summing up, then it appears that—

"The Trapezoidal and Arithmetic Mean Rules both err in defect in general", (18).

[The Errors due to adoption of these Rules are of course small. Still they are known to be constantly in one direction: as sufficiently simple and more accurate Rules are available, there can be no excuse for not adopting them].

3. Present Discharges.—The Discharges past the verticals were computed by the above formulæ *separately for every* SET of Subsurface Velocity-work in these Experiments. These may be called FAIR DISCHARGE-MEASUREMENTS, (for the reason given in Ch. VI, 16.) The Results are shown in Col. 7 of all the Tables VII—XXVIII. The means at the foot of these columns in each SERIES are the AVERAGE DISCHARGES past the verticals in question freed in general from the Observers' personal equation (Ch. VI, 13).

[It must be observed that the formulæ for Discharge above given are in no way connected with the difficult question of the geometric figure of the Velocity-Curve, being simply the ordinary formulæ for Areas of curved figures in which a number of equidistant ordinates are known; and are therefore correct approximations independently of the question of whether the Velocity-Curve be a Parabola or not].

4. Errors in Result.—It will be seen that the DISCHARGE past a vertical is virtually made up of two factors, viz., depth and velocity. Any Error affecting either of course affects the computed Discharge.

i. DEPTH-ERROR.—An Error (ΔH) in the estimation of the depth (H) on any vertical of course affects the Result proportionately, so that—

"Error in Discharge = U . ΔH", ...(19).

[The Error due to this is presumably least in calm weather, and greatest in a high wind, (from the greater difficulty of determining the depth accurately (Ch. V, 15) in this case)].

ii. VELOCITY-ERROR.—Errors in the (average) velocity-measurements used arise from two sources, viz., 1°, Unsteady Motion; 2°, Instrumental defects.

4a. *Unsteady Motion.*—The effect on the computed Result is that the Discharge-Measurement (D) of a single SET can only be accepted as a FAIR AVERAGE (Ch. VI, 16) and not as a *good average* value.

4b. *Instrumental Errors.*—The *general* effect of Instrumental Errors on the Discharge-Measurement may be inferred from their effect on the figure of the Velocity-Curve, (Ch. X, 10, & Pl. XIX) thus—

CASE (*a*). *Greatest Velocity at the surface. Fig.* 5*a*—

"The Area of the Observation-Curve is > the Area of the true Curve, and therefore the Discharge-Measurement is > the true Discharge", ...(20).

CASE (*b*). *Greatest Velocity below the surface. Fig. 5b*—

"When the Maximum-Velocity Line is anywhere near the surface (below of course), it is clear that the preceding Result will still obtain", ... (21*a*), and that—"the Areas will approach to equality as the Maximum Velocity-Line gradually sinks, until that line attains a position (*a*A in *Fig. 5b*), in which the loss of area above Q is balanced by the gain of area below Q, so that the Areas of the two Curves are then equal, and the Discharge-measurement correct", ... (21*b*).

Also, "As the Line of Maximum Velocity sinks below this critical position, it is clear that the Area of the Observation-Curve falls short of that of the true Curve, because the loss of area above the point Q (where the curves intersect) will be greater than the gain of area below that point, so that the Discharge-Measurement is < the true Discharge", ... (21*c*).

[There is some reason to think that the limiting position of the Line (*a*A) of maximum velocity is about the depth $ba = \frac{1}{2}$H, because this is the depth for which the Area BAC = C*l*D, accurately on the Parabolic Theory hereafter explained, and therefore approximately whether the Curve be Parabolic or not].

These Results may be summed up as follows *for all cases*:—

"The Discharge-Measurement (D′) > the true Discharge (D) when the Maximum Velocity Line is at or near the surface, and approaches equality with it as that Line gradually sinks to a depth of about $\frac{1}{2}$H, when they become equal. As this Line sinks still further, the Discharge-Measurement falls short of the true Discharge, and the discrepancy increases as the Line of Max. Velocity sinks", ... (22).

or in symbols—

$D' > D$ from $Z = 0$ to $Z = \frac{1}{2}H$ (about), and $(D' - D)$ decreasing .... (22*a*).

$D' = D$ when $Z =$ about $\frac{1}{2}H$, ... (22*b*).

$D' < D$ when $Z > \frac{1}{2}H$, and $(D - D')$, increasing, ... (22*c*).

[The two sources of Error also affect the Result differently *according as different formulæ are used*. For discussion of this Error, *see* Ch. XIX, 11].

5. PRACTICAL BEARING.—The only practical importance of this quantity is the aid it gives as being a first Step towards the computation of the all important Hydraulic quantity the TOTAL DISCHARGE of a channel, as will be explained in Chap. XIX.

It will be obvious that the tediousness of the Field-work required for obtaining the Discharges past many verticals by the Method above used, (viz., by numerous velocity-measurements at one-foot intervals on each vertical) would *preclude its practical use* in finding the Total Discharge. Practical convenience imperatively requires the discovery of some more rapid process for finding the Discharges past each vertical. This is the aim of the next three Chapters.

The Field-work process here used gives an *undeniable approximation.* This value will be used for *testing the sufficiency of the approximation* of approximate values obtained by far more rapid Field processes. And in this view the work so done will have been of great value.

CHAPTER XIV.

MEAN VELOCITY PAST A VERTICAL.

Preface.—This Chapter contains full details of the measurement of Mean Velocity past a vertical (Art. 1—7), with investigation and discussion of rapid approximation to it (Art. 8—10, 12, 13), and of its dependence on the External Conditions, (Art. 11—11b). Readers not interested in the mathematical developments should omit Art. 6b, 9, 9a, 9b, 9d.

1. **Mean Velocity past a vertical.**—This has been already defined (Ch. IV, 3) as—

"The Mean of the 'forward velocities' at all points of a Vertical".

It seems clear that its *proper* value is—

"The quotient of the Superficial Discharge (D) past the vertical by the depth, *i.e.*, $U = D \div H$",..(1).

The practical importance of this quantity, and of finding some means of a rapid approximation to it, will be explained hereafter, (Art. 8.)

[Some shorter and yet distinctive name for this important and frequently recurring quantity is badly wanted. Various names are used in different Works, *e.g.*—

Miss. Report, pp. 254, 294, Mean Velocity of the (whole) vertical curve.
p. 292, Mean of all the velocities in any vertical plane.
pp. 293, 294, Mean Velocity in any vertical plane.

Basin Expts., p. 236, Vitesse moyenne sur la verticale.

Lowell Expts., p. 146, Mean velocity of the water in the particular path followed by the tube.
pp. 146, 147, Mean velocity of the current, (or stream.)

Connecticut Report of 1878, { pp. 311, 313, 352, Mean velocity of (or in) the (vertical) section.
p. 313, Mean velocity in each curve.
p. 321, *et seq.*, Mean velocity in vertical plane.

Jackson's Hydraulic Manual, p. 82, Mean vertical velocity.

Weisbach's Mechanics, Vol. I, p. 509, Mean velocity of (or in) a perpendicular.

Most of these terms are either inconveniently long, or else open to objection on score of indistinctness or even of inaccuracy; thus—

1°. Jackson's term has the merit of shortness, but implies that the velocity itself is *vertical*.

2°. Several of the terms involving the phrases "in a vertical plane", or "in (or of) the section" are indistinct—at any rate when separated from the context—as they might be held to refer to the cross-section plane, and thus become confused with the "Mean Velocity through the cross-section".

3°. Bazin's term is inconveniently long, but seems sufficiently distinctive, and has accordingly been adopted in these Experiments with above translation.

To relieve the tedium of the periphrasis, the short term MEAN VELOCITY will frequently be used when the context prevents liability of confusion with other Mean velocities].

2. *Arithmetic mean too small.*—Some Experimenters have been content to take the arithmetic mean of the velocity-measurements at the several points on the same vertical as the value of the mean velocity past that vertical; it is of course an approximation, and when the number of points of velocity-measurement on the same vertical is *very numerous*, (which is rarely the case,) it is a good approximation but *not otherwise.*

[This course seems to have been adopted in the Mississippi Report (*see* Tables, pp. 247 to 251), and—to a large extent—in the Connecticut Report (*see* p. 313)].

Its sole merit consists in the ease of application, (compared with the somewhat laborious calculation by the proper formula $U = D \div H$); but when there are only a few points of velocity-measurement on the same vertical, it gives but a poor approximation. It is easy to see that, in consequence of the property before shown (Ch. X, 8) of the Average Vertical Velocity-Curve being generally *convex* down-stream,—

"The Arithmetic Mean of velocities at different points upon the same vertical is *always less* than the true Mean Velocity ($D \div H$) past that vertical", ...(2).

and it follows also obviously from Ch. XIII, 2d.

3. *Mean Velocity Variation small.*—It is clear that the Mean Velocity past a vertical being the mean of all velocities past that vertical, its variation from instant to instant must be some sort of mean of the variations of the individual velocities, so that in fact—

"The Mean Velocity past a vertical is less variable from instant to instant than most of the individual velocities of which it is the mean",........... ....(3).

[An Examination of the line (δ) of "Ranges" of the velocities throughout the Detailed Tables VII—XXVIII will amply confirm this; observing that the Tables show two *experimental* approximations (U, u) to the mean velocity].

4. *Mean velocity not constant.*—It is indeed quite possible that the variations of the individual velocities past a vertical might nearly balance, so as to leave the Mean Velocity past the vertical nearly* constant. As far as can be judged from the present Experiments this is not the case, or in other words—

"The Mean Velocity past a vertical varies sensibly from instant to instant",(4).

[The Detailed Tables VII—XXVIII give two Experimental approximations (U, u) to this Mean Velocity. A glance down the columns of U, u therein will show at once considerable variations in magnitude even in successive SETS in the same SERIES. The Average values and the "Ranges" of U, u for each Series are also

* This opinion was hazarded in the 1874-5 Report, (Art 58,) but it seems more probable now that the evidence was insufficient. It seems also to have been assumed in the Mississippi Report, (*see* Ch. X, 12a of present Work).

given in the Abstract Tables 3, 4. The "Ranges" can be seen to be *pretty large fractions* of the whole quantity, even as high as,—

Range of U, ·75 in 4·32, or 17·4 per cent. } in Ser. 9.
Range of *u*, ·78 in 3·98, or 19·6 per cent. }

This evidence is not so strong in either case, as it appears at first sight, inasmuch as *much of this variation* can, in both cases, be traced to change of External Conditions, &c. Perhaps the only really fair Test is the comparison of *successive Results of the same day's work*, in which it may be fairly assumed that the External Conditions (except Wind) remained tolerably constant. There are many cases of this sort, (viz., of several Sets of work in one day) throughout the Detailed Tables, the discrepancies between which are so great as to fairly establish the above Result].

5. PRESENT MEAN VELOCITIES.—The values of the (superficial) Discharges past the experimental verticals computed separately for each SET of Subsurface-velocities past those verticals, and shown in Col. 7 of Tab. VII to XXVIII, enable the Mean Velocity past those verticals to be computed for each SET by the formula $U = D \div H$. This has been done separately for each SET shown in these Tables, and the Result is shown in the Sub-column (U) throughout. And since the Discharges have been in every case computed by the best approximation-formulæ extant, the Mean Velocities in question are the *best approximations* to the true values that *could be obtained from the data*: also the Means at the foot of the Sub-columns are the AVERAGE MEAN VELOCITIES past each vertical nearly freed from the Observers' personal equation (Ch. VI, 13).

6. **Mean Velocity Error.**—The value of Mean Velocity obtained as above is of course liable to error similar to the Discharge-measurement (D) from which it is derived. But as explained in Ch. IV, 4a, the effect of error in estimation of the depth (H) on the vertical is almost wholly eliminated, so that the residual error is sensibly only that due to error in the primary Velocity-measurements on which the Discharge-measurement (D) depends.

This Error is of the same character as, and is also proportional to, the similar error in the Discharge-measurement, since $U = D \div H$, and arises therefore (Ch. XIII, 4, ii) from two sources, viz., (1), Unsteady Motion; and (2), Instrumental defects.

6a. *Unsteady Motion.*—In consequence of the Unsteady Motion, a single Mean Velocity-measurement (*i.e.*, that obtained from a single SET) cannot be considered a good average value, but only a FAIR AVERAGE, (*see* Ch. VI, 16.)

6b. *Instrumental Error.*—Referring to the similar Error in the Discharge-measurement previously investigated, (Ch. XIII, 4b) it follows :—

"The Mean Velocity-measurement (U') is > the true Mean Velocity (U) when the Maximum Velocity-Line is at or near the surface, and approaches equality with it as that Line gradually sinks to a depth of about ½H when they become equal; as this Line sinks still further, the Mean Velocity-measurement (U') falls short of the true Mean Velocity, and the Discrepancy increases as the Maximum Velocity-Line sinks",..(5),

or in symbols—

$U' > U$ from $Z = 0$ to $Z =$ about $\frac{1}{4}H$, and $(U' - U)$ decreasing,................(5a).

$U' = U$ when $Z =$ about $\frac{1}{4}H$,..(5b).

$U' < U$ when $Z > \frac{1}{4}H$, and $(U - U')$ increasing with Z, ...(5c).

Now, since Z is seldom $> \frac{1}{4}H$, *see* Abstr. Tab. 3, 4, (except on verticals near the margin of a rectangular section,) it follows that :—

"The Mean Velocity-measurement (U') is as a general rule > the true Mean Velocity (U)",...(6).

The application of these Rules (5) in practice would require a knowledge of the value of Z, which cannot be found (Ch. XI) without a good deal of labor. From the property (Ch. XI, 7) that the Average Vertical Velocity Curve is at any rate approximately a common parabola, they may be put into a more convenient form.

For, by the property (39) of the common parabola shown in Art. 9d,

$U <=> v_0$ when $Z <=> \frac{1}{4}H$, ...(7).

Hence $U' >=< U$ when $U <=> v_0$, ...(8a).

Or, in other words, $(U' - U)$ is of opposite sign to $(U - v_0)$, ... (8b).

Hence also by the principles of infinitesimals, these differences being small,

$U' >=< U$ when $U' <=> v_0$, ...(9a).

and $U' - U$ is of opposite sign to $U' - v_0$, ...(9b),

or in words—

"The Mean Velocity-measurement (U') exceeds or falls short of the true Mean Velocity (U) according as it is less or greater than the Surface-velocity (v_0)",...(9).

[The importance of this last Result will be better seen when discussing the approximation to Mean Velocity given by use of loaded Rods, (Ch. XV, 8e)].

7. **Practical Bearing.**—The only practical use proposed to be made of the value of the Mean Velocity above obtained is to use it as a TEST of the sufficiency of the approximation of certain modes of *rapidly* obtaining an approximate value thereof about to be proposed.

8. **Approximation, IMPORTANCE OF.**—It is obvious that if an approximate value of the Mean Velocity (U) past the vertical could be obtained by any *rapid* process in the Field, it would serve far better for practical purposes for calculation of the Discharge past a vertical by the fundamental formula—

$D = U . H$, ... (10),

than the process used in last Chapter depending on the tedious Field-work of velocity-measurements at one-foot intervals.

The Mississippi Experimenters give—as the result of their experience—the following important recommendation, (p. 292 of Report) :—

"It seems, therefore, that efforts should be directed to simplifying the determination of the mean of all the velocities *in any vertical plane*",

i. e., the MEAN VELOCITY PAST A VERTICAL. This seems to the Author one of the most important Results in the Mississippi Report. Great atten-

tion was therefore paid to this point in these Experiments: *the whole of the Subsurface work was in fact arranged with a view to elucidating this,* and *all that precedes in this Work may be said to lead up to this.*

9. Parabolic Formulæ.—It seems natural to inquire first whether the Mean Velocity past a vertical can not be found from velocity-measurements *at only two or three points* on that vertical. And here considerable aid may be derived from study of the Velocity-Parabola. Whether the Vertical Velocity-Curve be really a common parabola or not matters little: it must be admitted that it does certainly approximate (*see* Ch. XI, 7) to a parabola. This approximation is quite sufficient to admit of its use in determining an approximate value of Mean Velocity.

And first, it is clear that, as three data suffice to determine the velocity-parabola completely, velocity-measurements at three distinct points on the same vertical will of course suffice to determine the Mean Velocity.

[The three points must of course be suitably situate to give a tolerably accurate determination].

The first Step is to find an expression for the Mean Velocity. Adopting the well known property—

Area of parabola between tangent and diameter $= \frac{1}{3} \times$ circumscribing rectangle, (11),

it follows easily that, (referring to Pl. XX, 3)—

Discharge through depth H, or Area BADdb, $\Big\}$ D = Rectangle $b'd$ − sum of parabolic areas (Bb'A + Ad'D),

$= \text{Rectangle } b'd - \frac{1}{3} \text{ rectangle BA} - \frac{1}{3} \text{ rectangle AD},$

$$= \text{VH} - \tfrac{1}{3}(\text{V} - v_0)\,.\,\text{Z} - \tfrac{1}{3}(\text{V} - v_{\text{H}})\,.\,(\text{H} - \text{Z}), \;\ldots\ldots(12).$$

Writing the equation of the curve in the form (Ch. XI, 2, (2))—

$$\text{V} - v = m\,(z - \text{Z})^2, \text{ where } m = \frac{1}{p} \ldots\ldots\ldots\ldots(13),$$

and writing $z = 0$, $z = \text{H}$ in succession therein (so that v becomes v_0 and v_{H})

$$\text{V} - v_0 = m\text{Z}^2, \text{ and } \text{V} - v_{\text{H}} = m\,(\text{H} - \text{Z})^2, \ldots\ldots\ldots\ldots(14).$$

Substituting these into the expression (12) for Discharge

$$\text{D} = \text{VH} - \tfrac{1}{3}\{m\text{Z}^3 + m\,(\text{H} - \text{Z})^3\}$$
$$= \text{VH} - \tfrac{1}{3}m\text{H}^3 + m\text{H}^2\text{Z} - m\text{HZ}^2, \ldots\ldots\ldots\ldots(15).$$

$$\therefore \text{ Mean Velocity } \text{U} = \frac{\text{D}}{\text{H}} = (\text{V} - m\text{Z}^2) + m\text{HZ} - \tfrac{1}{3}m\text{H}^2,$$
$$= v_0 + m\text{HZ} - \tfrac{1}{3}m\text{H}^2, \ldots\ldots\ldots\ldots(16),$$

by substituting from (14). This is the working expression for U, with which other values obtained in terms of observed velocities are to be compared.

9a. *Three-Velocity Formulæ.*—Now let three velocity-measurements $v_{\lambda\text{H}}, v_{\mu\text{H}}, v_{\nu\text{H}}$ be taken at *any* depths λH, μH, νH, (where λ μ, ν are proper fractions,) and let it be proposed to find an expression for the Mean Velocity in terms of these; let this be—

$$\text{U} = \alpha\,.\,v_{\lambda\text{H}} + \beta \cdot v_{\mu\text{H}} + \gamma \cdot v_{\nu\text{H}}, \ldots\ldots\ldots\ldots(17),$$

where α, β, γ are numerical co-efficients to be determined.

Subtracting (13) from (14), there results the following general expression for v :—

$$v = v_0 + 2mZz - mz^2, \text{......................................}(18).$$

Writing $z = \lambda H, \mu H, \nu H$ in succession, this gives—

$$v_{\lambda_H} = v_0 + 2mZ.\lambda H - m\lambda^2H^2, \quad v_{\mu_H} = v_0 + 2mZ.\mu H - m\mu^2H^2,$$

and
$$v_{\nu_H} = v_0 + 2mZ.\nu H - m\nu^2H^2, \text{......................................}(19).$$

Multiplying by α, β, γ in succession, and adding, it follows from (17) that—

$$U = (\alpha+\beta+\gamma).v_0 + 2mHZ(\alpha\lambda+\beta\mu+\gamma\nu) - mH^2(\alpha\lambda^2+\beta\mu^2+\gamma\nu^2), \text{......}(20).$$

This expression becomes identical with (16) by making—

$$\alpha+\beta+\gamma=1, \quad \alpha\lambda+\beta\mu+\gamma\nu=\tfrac{1}{2}, \quad \alpha\lambda^2+\beta\mu^2+\gamma\nu^2=\tfrac{1}{3}, \text{..................}(21).$$

These being simple equations in α, β, γ suffice to determine α, β, γ in terms of λ, μ, ν *always*, *i. e.*, whatever λ, μ, ν be). The general solution is not of much practical use: the most useful particular solutions appear to be when the three velocity-measurements are made at mid-depth ($\mu H = \frac{1}{2}H$) and at two points equidistant from mid-depth (in which case $\lambda H + \nu H = H$), so that—

$$\mu = \tfrac{1}{2}, \quad \lambda+\nu = 1, \text{..}(22),$$

which reduce (21) to—

$$\alpha+\beta+\gamma=1, \quad \alpha\lambda+\tfrac{1}{2}\beta+\gamma\nu=\tfrac{1}{2}, \quad \alpha\lambda^2+\tfrac{1}{4}\beta+\gamma\nu^2=\tfrac{1}{3}, \text{..................}(23).$$

Multiplying the last two by 2 and 4 respectively, and subtracting in turn from the first,—

$$\alpha(1-2\lambda)+\gamma(1-2\nu)=0, \quad \alpha(1-4\lambda^2)+\gamma(1-4\nu^2)=-\tfrac{1}{3}, \text{..............}(24).$$

Substituting $\lambda+\nu$ for 1 into the former,—

$$(\alpha-\gamma)(\nu-\lambda)=0; \text{ whence } \alpha=\gamma \text{ (as } \nu, \lambda \text{ are supposed unequal)}, \text{......}(25).$$

And from the latter, $2\alpha.\{1-2(\lambda^2+\nu^2)\}$, or $2\alpha\{(\lambda+\nu)^2-2(\lambda^2+\nu^2)\}=-\frac{1}{3}$

$$\text{whence,} \quad \alpha=\gamma=\frac{1}{6(\lambda-\nu)^2}, \quad \text{or} = \frac{1}{6(2\lambda-1)^2}, \text{..................}(26a).$$

$$\beta = 1-2\alpha = 1-\frac{1}{3(\lambda-\nu)^2}, \quad \text{or} = 1-\frac{1}{3(2\lambda-1)^2}, \text{..............}(26b).$$

RESULTS. Hence the following simple cases (assigning simple values 0, $\frac{1}{6}$, $\frac{1}{4}$, $\frac{1}{3}$ to λ),

$$U = \tfrac{1}{6}(v_0+4v_{\frac{1}{2}H}+v_H), \quad \text{or} = \tfrac{1}{8}(3v_{\frac{1}{6}H}+2v_{\frac{1}{2}H}+3v_{\frac{5}{6}H}), \text{............}(27a).$$

$$= \tfrac{1}{3}(2v_{\frac{1}{4}H}-v_{\frac{1}{2}H}+2v_{\frac{3}{4}H}), \quad \text{or} = \tfrac{1}{2}(3v_{\frac{1}{3}H}-4v_{\frac{1}{2}H}+3v_{\frac{2}{3}H}), \text{..................}(27b).$$

[The first will be recognized as Simson's well known formula: it is *of no use for practical determination* of U, as it involves the bed-velocity which does not admit of direct measurement. The other three give simple values, easily applicable to practical velocity-measurement].

9b. *Two-Velocity Formulæ.*—There being *only three* equations (21) connecting the six quantities $\alpha, \beta, \gamma, \lambda, \mu, \nu$, it seems worth while to inquire whether an expression could be found for the Mean Velocity involving velocity-measurements at only two (instead of three) distinct points, as this would materially reduce the Field-work necessary to find the Mean Velocity.

It is sought then to determine $\alpha, \beta, \lambda, \mu$ so as to determine U by the simpler formula—

$$U = \alpha v_{\lambda_H} + \beta v_{\mu_H}, \text{......................................}(28).$$

Either by a similar investigation to the preceding, or by simply writing $\gamma = 0$ in

the previous Result (21), the equations connecting a, β, γ are seen to be

$$a+\beta=1,\quad a\lambda+\beta\mu=\tfrac{1}{2},\quad a\lambda^2+\beta\mu^2=\tfrac{1}{3},\ \ldots\ldots\ldots\ldots(29),$$

from which it is clear that λ, μ are no longer independent; for solving for a, β in the two first,

$$a=\frac{\mu-\frac{1}{2}}{\mu-\lambda},\quad \beta=\frac{\frac{1}{2}-\lambda}{\mu-\lambda},\ \ldots\ldots\ldots\ldots(30).$$

Substituting into the third, $\frac{1}{2}\lambda^2-\mu\lambda^2+\mu^2\lambda-\frac{1}{2}\mu^2=\frac{1}{3}(\lambda-\mu)$, and dividing by $(\lambda-\mu)$, (which is always possible, since λ, μ must be unequal)—

$$\lambda\mu-\tfrac{1}{2}(\lambda+\mu)+\tfrac{1}{3}=0,\ \ldots\ldots\ldots\ldots(31),$$

which is the equation connecting λ, μ, from which in fact

$$\lambda=\frac{\frac{1}{2}\mu-\frac{1}{3}}{\mu-\frac{1}{2}},\quad\text{or }\mu=\frac{\frac{1}{3}-\frac{1}{2}\lambda}{\frac{1}{2}-\lambda},\ \ldots\ldots\ldots\ldots(32),$$

so that either is determined in terms of the other.

Thus the Mean Velocity (U) may be found from velocity-measurements at *only two* distinct depths λH, μH—whereof one is arbitrary, and the other is determined by (32)—by the simple formula (28), wherein a, β are given by (30).

RESULTS. Hence the following simple cases, (found by making $\lambda=0,\frac{1}{6},\frac{1}{4},\frac{1}{3}$)

$$U=\tfrac{1}{4}(v_0+3v_{\frac{2}{3}H}),\quad\text{or}=\tfrac{1}{7}(3v_{\frac{1}{6}H}+4v_{\frac{3}{4}H}),\ \ldots\ldots\ldots\ldots(33a).$$

$$U=\tfrac{1}{7}(4v_{\frac{1}{4}H}+3v_{\frac{5}{6}H}),\quad\text{or}=\tfrac{1}{4}(3v_{\frac{1}{3}H}+v_H),\ \ldots\ldots\ldots\ldots(33b).$$

These are the simplest* formulæ by which the Mean Velocity past a vertical can be determined from velocity-measurements at *only two* distinct points. The last is of no practical use, as it involves v_H a quantity which cannot be practically measured.

The first of the formulæ (33a) above* is *by far the best for general purposes*, because it involves only one *subsurface* velocity ($v_{\frac{2}{3}H}$), and that at the *highest possible* level ($\frac{2}{3}$H), and therefore admitting of more accuracy in its determination than those at lower levels involved in the other formulæ.

[It is not difficult to show that the two velocity-measurements must always lie one in the upper third, and one in the lower third of the depth, *i.e.*, λ lies between 0, $\frac{1}{3}$, and μ between $\frac{2}{3}$ and 1].

9c. PRESENT WORK, *Application*.—It is proposed for distinctness' sake to denote the value of Mean Velocity derived from the above simple formula (first of 33a) by u_m, *i.e.*, to write—

$$u_m=\tfrac{1}{4}(v_0+3v_{\frac{2}{3}H}),\ \ldots\ldots\ldots\ldots(33a,\ bis).$$

The value of this quantity has been calculated for all the 46 Average Vertical Curves of the present Experiments, and is shown in the Sub-Column headed u_m in Abstr. Tab. 3, 4 for comparison with the fundamental value U = D ÷ H. To facilitate this, the Discrepancy (u_m − U) is also shown in Col. 9. These Discrepancies will be seen to be always small (nowhere exceeding ·07) as might be expected, and usually negative, showing that u_m < U usually.

[The closeness of the values of u_m, U is involved of course in the general approximation of the Observation-Curves to Parabolæ].

9d. Depth of Mean Velocity-Line.—By the term "Line of Mean Velocity" is here meant the Stream-Line in which the Average Forward Velocity is equal to

* Now published for the first time it is believed.

the Average Mean Velocity past the vertical. To find the depth (h_0) of that Line, the equation of the Curve (18) gives, (writing $s = h_0$, and $v = U$)—

$$U = v_0 + 2mZh_0 - mh_0^2, \quad\text{.......................................} (34a).$$

$$= v_0 + mZH - \tfrac{1}{3}\,mH^2, \text{ by Result (16),} \quad\text{....................} (34b).$$

Hence $\qquad h_0^2 - 2Zh_0 = \tfrac{1}{3}H^2 - ZH,$

whence $$h_0 = Z \pm \sqrt{\tfrac{1}{3}H^2 - ZH + Z^2}, \quad\text{..................................} (35).$$

and $$\frac{h_0}{H} = \frac{Z}{H} \pm \sqrt{\frac{1}{3} - \frac{Z}{H} + \left(\frac{Z}{H}\right)^2}, \quad\text{...............................} (35a).$$

The quadratic in h_0 has of course two roots: but it is easily seen by writing (35) in form—

$$h_0 = Z \pm \sqrt{H\,(\tfrac{1}{3}H - Z) + Z^2}, \quad\text{..............................} (35b),$$

that one root is always negative when $Z < \frac{1}{3}H$, and is therefore of no* interest: when $Z > \frac{1}{3}H$, both roots are +, which shows that there are in this case two Lines of Mean Velocity equidistant from the axis, (as is evident from the symmetry of the parabola.) It may be shown also that the larger root is always greater than $\frac{1}{2}H$, for writing the larger root of (35) in form—

$$h_0 = Z + \sqrt{(\tfrac{1}{2}H - Z)^2 + \tfrac{1}{12}H^2}, \quad\text{....................................} (35c),$$

so that $\qquad h_0 = Z + \text{a quantity} > \frac{1}{2}(H - Z)$, whence $h_0 > \frac{1}{2}H$, ... (35d),

which shows that—

"The Mean Velocity Line is always below the mid-depth", ... (36).

[For illustration of this, *see* Pl. XII—XVIII. The vertical line drawn through the tip of the Mean Velocity Ordinate (U) will be seen to cut the Observation-Curves in almost all cases below the mid-depth].

It will be seen that the depth of the Mean Velocity-Line (defined by h_0), depends on the position of the maximum velocity line (defined by Z), and varies therefore with the variation of the latter; also from Eq. (35a) it follows that:—

"The relative depth of the Mean Velocity Line ($h_0 \div H$) depends solely on the relative depth of the maximum velocity line ($Z \div H$)", ... (37a).

The Range of the maximum velocity line appears to be from a little above the surface down to about mid-depth, (*see* Pl. XII—XVIII.) The values of h_0 corresponding to various values of Z within this range are shown below.

Value of $Z \div H$,	$-\frac{1}{4}$,	$-\frac{1}{6}$,	0,	$\frac{1}{8}$,	$\frac{1}{6}$,	$\frac{1}{4}$,	$\frac{1}{3}$,	$\frac{1}{2}$,
Value of $h_0 \div H$,	·554,	·560,	·577,	·598,	·607,	·632,	0 & ·667,	·211 & ·789.

From the above, it follows that—

"The Mean Velocity past a vertical *cannot be directly measured* in practice by any single velocity-measurement", ... (37b),

because the measurement would have to be effected in the Mean Velocity Line, a Line whose position is not known *à priori*.

Again, taking the larger root of (35) (which is the one of most interest), viz.,

$$h_0 = Z + \sqrt{(\tfrac{1}{3}H - Z)\,H + Z^2}, \quad\text{..................................} (35, \textit{bis}),$$

it is clear that the surd is $> = <$ Z when $\frac{1}{3}H > = <$ Z,

$$\therefore\ h_0 > = < 2Z \text{ when } Z < = > \tfrac{1}{3}H, \quad\text{................................} (38).$$

* as this would correspond to a line *above the surface*.

Now from the symmetry of the curve it is clear that the velocity (v_{2Z}) at depth $z = 2Z$ is the same as the Surface velocity, *i.e.*, $v_{2Z} = v_0$.

Hence—

The Mean Velocity (U) $> = <$ the Surface Velocity (v_0) when $Z > = < \frac{1}{3}$ H, (39).

9e. ***Single Velocity Approximations.***—Writing down the general values of U, v from Eq. (16), (18),

$$v = v_0 + 2mZz - mz^2, \quad U = v_0 + mZH - \tfrac{1}{3}mH^2, \text{ ....(40)},$$

it is manifest that there is no value of z (taken as a function of the depth (H) only) which will make the general value of v either equal to U, or even proportional to U, in consequence of the presence of the variable and unknown Z. The flatness of the Velocity-Parabolæ is, however, in all cases so great (Ch. X, 8) that *an approximation is possible.* The closeness of this approximation depends on a prior rough knowledge of the Range of $Z \div H$. Now a glance down the column (Tab. 3, 4) showing the values of $Z \div H$ in the 45 Curves of the present Experiments will show that the Range of this quantity is—except for verticals quite close to the vertical walls of the rectangular channel (*i.e.*, for all verticals more than 5′ off the walls)—only from about 0 to $\frac{1}{4}$, and for this range of $Z \div H$, the value of $h_0 \div H$ has been shown (Art. 9d) to range from ·577 to ·667 ; with a mean value of about ·625 = $\frac{5}{8}$.

Now the velocity corresponding to the value $z = \frac{5}{8}$ H is from (40),—

$$v_{\frac{5}{8}H} = v_0 + m\left(\frac{5}{4}ZH - \frac{25}{64}H^2\right), \text{ ....(41)},$$

and the difference between this and the Mean Velocity is—

$$v_{\frac{5}{8}H} - U = \tfrac{1}{4}m\left(ZH - \frac{11}{48}H^2\right), \text{ which ranges}$$

$$\text{from } -\frac{11}{192}mH^2, \text{ (when } Z = 0) \text{ to } + \frac{5}{192}mH^2, \text{ (when } Z = \tfrac{1}{3}H), \text{.......(41a)}.$$

CASE ii. Near the margin of the rectangular channel on the other hand the quantity $Z \div H$ lies between $\frac{1}{4}$ and $\frac{1}{2}$, (*see* Abstr. Tab. 3, 4,) and the Table of values of $h_0 \div H$ (Art. 9d) shows that there are *two* values of $h_0 \div H$ corresponding, viz., one between 0 and ·211, and one between ·667 and ·789, with mean values of about ·105 and ·728. The former is the better for practical velocity-measurements on account of the greater accuracy of work near the surface.

Now the velocity corresponding to the value $z = \frac{1}{10}$ H is

$$v_{\frac{1}{10}H} = v_0 + m\left(\frac{1}{5}ZH - \frac{1}{100}H^2\right) \text{ ....(42)},$$

and the difference between this and the Mean Velocity is—

$$v_{\frac{1}{10}H} - U = \tfrac{1}{5}m\left\{-4ZH + \frac{97}{60}H^2\right\}, \text{ which ranges}$$

$$\text{from } \frac{17}{300}mH^2, \text{ (when } Z = \tfrac{1}{4}H) \text{ to} - \frac{23}{300}mH^2, \text{ (when } Z = \tfrac{1}{2}H), \text{....... (42a)}.$$

Now, in consequence of the flatness of all the Curves the quantity m (= reciprocal of parameter) is always a very small quantity ; so that—

"the several Discrepancies $-\frac{11}{192}mH^2, \frac{5}{192}mH^2, \frac{17}{300}mH^2, -\frac{23}{300}mH^2$, just shown are *always very small quantities*", ...(43),

and :

"The two Velocities $v_{\frac{3}{5}H}$ (*i.e.*, at $\frac{3}{5}$ depth) in general, and $v_{\frac{1}{10}H}$ (*i.e.*, at $\frac{1}{10}$ depth) near margin of a rectangular channel are probably the *best approximations* obtainable from velocity-measurement at a single point", ...(44).

9f. *Mid-depth-velocity*, ($v_{\frac{1}{2}H}$.—Writing $s = \frac{1}{2}$H in the general expression (18) for v, the Mid-depth Velocity is seen to be,—

$v_{\frac{1}{2}H} = v_0 + m\text{ZH} - \frac{1}{4}m\text{H}^2$, whilst $\text{U} = v_0 + m\text{ZH} - \frac{1}{3}m\text{H}^2$, (by (16)),........(45),

so that the difference $v_{\frac{1}{2}H} - \text{U} = \frac{1}{12}m\text{H}^2$, (always a + quantity),.................(46).

Thus in the Velocity-Parabola—

"The Mid-depth-Velocity is always > the Mean Velocity by a small quantity viz., $\frac{1}{12}m\text{H}^2$, not depending on the position of the axis", ... (46*a*).

It will be seen also that the Discrepancy $\frac{1}{12}m\text{H}^2$ is *always* > the *greatest possible* Discrepancies with the two approximations last proposed.

[The property just proved, viz., that the "Mid-depth ordinate exceeds the Mean ordinate by a small quantity" is a property in no way peculiar to the parabola. All Experiment agrees in showing that as a rule, (*see* Ch. X, 8)—

"The Average Vertical Velocity-Curves are 1°, everywhere convex down-stream; and 2°, are always very flat Curves".

These two properties involve the property in question: for in any convex curve whatever (*see* Pl. XX, 5), the tangent at the point M where the middle ordinate *m*M meets the curve lies wholly without the curve, so that the curve falls wholly within the circumscribing trapezoid; also,

Middle ordinate *m*M = Area of circumscribed trapezoid *bb*′M*d*′D ÷ depth *bd*,
Mean ordinate = Area of curve *b*BMD*d* ÷ depth *bd*, (by definition),

so that the Middle ordinate always > the Mean ordinate; also, when the curve is very flat, it is clear that the excess of the former over the latter must be a small quantity].

This is fully borne out by the present Experiments: the value of the quantity ($v_{\frac{1}{2}H} - \text{U}$) is shown for every SERIES in Abstr. Tab. **3, 4,** Col. **9,** and it will be seen from them that—

"The difference ($v_{\frac{1}{2}H} - \text{U}$) is always a small quantity, and usually +, so that $v_{\frac{1}{2}H}$ usually exceeds U",..(47).

[($v_{\frac{1}{2}H} - \text{U}$) is + in 40 out of the 46 Series, and zero in 2 more. The only cases in which $v_{\frac{1}{2}H} < \text{U}$ are shown in following Table].

Serial No.	Number of Sets.	Value of ($v_{\frac{1}{2}H} - \text{U}$).	Remarks.
9	14	−·07	Several very low velocities about the mid-depth (*i.e.*, at 4′ and 5′ depth), *see* Tab. XI.
21	16	−·01	An unimportant difference.
44	5	−·11	These two Curves on the exceptional vertical, close to the 4′ drop-wall, (*see* Cross-section, Pl. XVIII,) are of exceptional shape, (not wholly convex,) so that the property (47) of a convex curve *could not be expected*.
45	6	−·06	

10. *Ratio* $U \div v_{\frac{1}{2}H}$.—This ratio has acquired quite exceptional importance of late years from the assertion in the Mississippi Report (p. 294) of its approximate constancy *under all circumstances* at the same Site, and the proposal therein (p. 295) to utilize this supposed property in Discharge-measurement.

From the Result $v_{\frac{1}{2}H} = U + \frac{1}{12} mH^2$, Eq. (46), it is clear that the ratio $U \div v_{\frac{1}{2}H}$ is—in the Velocity-Parabola at any rate—not a *constant* quantity (unless mH^2 be proportional to U), nor a function of U only (unless indeed mH^2 be a function of U). The value of the ratio is in fact—

$$\frac{U}{v_{\frac{1}{2}H}} = \frac{U}{U + \frac{1}{12} mH^2} = \frac{1}{1 + \frac{1}{12} \cdot \frac{mH^2}{U}} \dots\dots\dots (48).$$

Now from the admitted smallness of the quantity $\frac{1}{12} mH^2$, (the same as $v_{\frac{1}{2}H} - U$) it is clear that this ratio will be *tolerably constant* (< 1, of course) at any rate *as a rough approximation.*

10a. *Mississippi Report Result.*—The Conclusion advanced is that this ratio depends chiefly on the mean velocity (V) of the channel, at any rate in a deep channel.

But the argument is based (Miss. Report, p. 293) upon the *assumed* value for the parameter $\frac{1}{m}$ or $p = H^2 \div \sqrt{\beta V}$, (Ch. XI, 11a), and upon a further *assumed* relation that $U = \cdot 93\ V$ approximately, (*i.e.*, with sufficient approximation for the purpose of proving the dependence of the ratio $U \div v_{\frac{1}{2}H}$ on V). Applying these two Results, the ratio $v_{\frac{1}{2}H} \div U$ indeed becomes—

$$v_{\frac{1}{2}H} \div U = 1 + \frac{1}{12 \times \cdot 93}\sqrt{\frac{\beta}{V}}, \text{ where } \beta = \frac{1\cdot 69}{\sqrt{H + 1\cdot 5}}, \text{ (Ch. XI, 11a),} \dots (49),$$

which depends in deep channels at any rate (in which β varies very little) chiefly on V: and this Result is proposed (p. 293) as—

"the absolute numerical value of the ratio for any curve of actual observations".

But the *Argument* is inconclusive on account of the uncertainty (and probable incorrectness as *general* truths) of the two assumptions $p = H^2 \div \sqrt{\beta V}$ and $U = \cdot 93\ V$ approximately. The assumption $U = \cdot 93\ V$ approximately is obviously *not true at all parts* of a channel, for it is equivalent to *assuming* that—

> "The mean velocity past a vertical (U) is approximately the same right across a channel",

which is true enough throughout great part of the width, but *very far from true near the banks,* as will abundantly appear in Chap. XVII. Thus Result (49) is not a *general* truth, but is at the utmost *limited in application to those parts* of a cross-section, the mean velocity past the verticals of which is nearly the same.

In fact the real evidence of the proposed law for this ratio must be held to depend, not on the argument which led to it, but, on the numerical comparisons exhibited (Miss. Report, p. 294) showing—

1°, the values of the ratio $U \div v_{\frac{1}{2}H}$ (computed direct from the velocity-data).

2°, the values of its proposed equivalent, viz., of $1 \div \left(1 + \frac{1}{11{\cdot}16}\sqrt{\frac{\beta}{V}}\right)$.

3°, the Discrepancies between the above values.

These are shown (Miss. Report, p. 294) for 15 Cases, viz., 8 Mississippi Curves, 2 of Capt. Boileau's Curves from small canals, and 5 Curves on the Rhine. The DISCREPANCIES shown are certainly surprisingly small in the 8 Mississippi Curves, in which they do not exceed $\frac{2}{10}$ per cent.; whilst in 4 of the European Curves they rise to 2 to 3 per cent.

Upon this evidence the important Conclusion is drawn (*ib.*) that—

"The ratio of the mid-depth velocity to the mean velocity in any vertical plane is practically independent of the depth and the width of the stream, of the mean velocity of the river, of the mean velocity of the vertical curve, and of the locus of its maximum velocity. In other words, it is a sensibly constant quantity for practical purposes".

And upon this Conclusion it is proposed (*ib.* pp. 295, 296) that the Field-work for computing the Total Discharge of a large channel should in future be limited to mid-depth velocity-measurements.

The practical value of this Conclusion depends chiefly on the amount of Error likely to be made in its application. Now the value of the ratio (49) proposed involves unfortunately the unknown quantity V (= mean velocity of channel). If an *approximate* value of this were known *à priori*, it would give the value of the ratio in question with sufficient approximation.

It was apparently supposed (Miss. Report, p. 294) that the ratio in question varied within such small limits *under all circumstances whatever* (even in different channels) that it might (for all practical purposes of Discharge-Measurement of large channels) be assumed sensibly constant. The additional evidence now available by no means confirms this hypothesis: the ranges of average values of the ratio in question—*i.e.*, of the average experimental values of $U \div v_{\frac{1}{2}H}$—are given below from all the known published cases.

EXPERIMENTS.	Reference to Original.	Number of Curves.	Range of Average Values of the ratio $U \div v_{\frac{1}{2}H}$.
Mississippi, ...	Miss. Report, p. 294,	8	·9868 to ·9624
Rhine, ...	" "	5	·9569 to ·9322*
Small Canals, Capt. Boileau,	" "	2	·9640 to ·9417
Bazin, ...	Bazin Expts.,	?	not given
Lake Survey, ...	Reports of 1868-70,	?	not given
Irrawaddi, ...	Report of 1875, Appx. C.	14?	1·082 to ·976
Connecticut, ...	Report of 1878, p. 350,	27?	·961 to ·918
Roorkee, ...	Roorkee Expts., Tab. 3, 4,	46	1·045 to ·961

Thus it appears that—

"The ratio $U \div v_{\frac{1}{2}H}$ is liable to range from about 1·082 to ·918, *i.e.*, about 16 per cent.", ... (50),

an amount not fairly negligible even in the rough process of Discharge-Measurement of large channels.

* printed ·0322 in Mississippi Report.

11. External Conditions, DEPENDENCE ON.—The dependence of the Mean Velocity past a vertical on the External Conditions is a question of the highest interest both theoretical and practical (as a Step towards finding the Total Discharge without the labor of velocity-measurement).

11a. *Preliminary Trial.*—In Pl. XXI, XXII the two approximate Mean Velocity-measurements (U, u) have been plotted as ordinates to the Average Depths (H) for all the 28 Vertical Curves on Central Verticals (Ser. 1—28) together with the Surface-Falls (F_1, F_2, F_3), parabolic elements (p, Z, ζ, V), and Average Winds corresponding. It will be at once seen that—

"The Mean Velocity past a central vertical increases and decreases on the whole (in absence of other influences) with increase and decrease of depth ",......... (51),

but the departures from this rule are so numerous and so great as to make it clear that it also depends on some other elements to a degree sufficient to quite mask and even reverse the effect of change of depth. And on comparing the curves of Surface-Fall (F_1, F_2, F_3), with the curves of U, u, it will be seen that—omitting Ser. 18—20 in the exceptional state of the Left Aqueduct being closed—the saliences and depressions of the curves of F_1, U, u concur with very few exceptions on the same ordinates; also that there is a partial concurrence of saliences and depressions of the curves of F_2, F_3 with those of U, u.

This shows that—

"The Mean Velocity past a central vertical increases and decreases on the whole *i. e., cæteris paribus* with increase and decrease of the Surface-Fall, and chiefly with the Surface-Fall in the Upper Sub-Reach ",..................................... (52).

11b. *Further Trials.*—It appears then that this Mean Velocity depends partly on both the Depth and Surface-Gradient, and is therefore a function of both of them. It seemed therefore worth while comparing the products HF_1, HF_2, HF_3, HF, their square roots,* &c., with the values of U. The Abstr. Tab. 8 and Pl. XLIII have been prepared to exhibit the Results.

The Table is arranged by order of decreasing Mean Velocity (U) at each Site as the Argument: this has the advantage that those of the Trial Quantities (HF, &c.), which are *in any way simply related* to the Argument (U), should at any rate be ranged in the same order. Thus the product HF_2 is seen to be so irregular, *see* Ser. 21—28, that the research is not worth pursuing with respect to F_2. Similarly the trial quantities HF, $\sqrt{HF}$ may be seen to be so irregular that the research may be dropped with respect to F.

[Similarly the product HF_3 was found to be so irregular that the research with respect to F_3 was at once dropped, and the Results were not thought worth publishing].

The *tolerable regularity* of decrease (*see* Tab. 8) in the trial quantities HF_1 $\sqrt{HF_1}$ seemed to make it worth while pursuing the research with respect to F_1 alone:

* The expressions of type $\sqrt{HF}$ were of course suggested by the analogy of the well known corresponding formula for Mean Sectional Velocity $V = C \times 100 \sqrt{RS}$; the Surface-Falls ($F_1$, F_2, F_3) being the nearest measure of the Surface-Slope (S) available with every velocity-experiment. (Ch. VII, 8c).

on comparing the Results HF_1, $\sqrt{HF_1}$ with the values of U, it is evident that changes in F_1 are *more important than those of* H, so that the quantity H should be involved in a lower degree than F_1. The following quantities were accordingly computed :—

$$\sqrt{HF_1},\quad F_1\sqrt{H},\quad F_1^2\sqrt{H},\quad F_1\sqrt[4]{H},\quad \sqrt{F_1\sqrt{H}},$$

as Trial Quantities, and are shown in the Table.

Pl. XLIII. To render the relation (if any) between the Trial Quantities and the Mean Velocity (U) more clear, these five quantities have been plotted as ordinates to the Mean Velocity (U) taken as abscissæ. This has the advantage that *any simple relation* would at once be exhibited by the form of the Curve of the Trial Quantity (simple proportion being indicated by a straight line through the origin, any simple linear relation by a straight line, any other simple relation by a fairly flowing Curve, &c., the non-existence of a simple relation by an irregular curve). On examining all the curves, it is at once seen that those curves in which F_1 is involved in a higher degree than H are the most regular; and that on the whole the curve of $F_1\sqrt[4]{H}$ is the most regular, and approximates to a straight line. The irregularities in all the Curves are, however, so great as to make it uncertain whether there is any real connexion, or whether the apparent connexion is not solely due to paucity of data. Thus, by omission of a few of the data here and there, any of the curves tried would be apparently a fairly flowing Curve, and therefore apparently related in some simple manner to the primary velocity (U).

On the whole it seems clear that—without some better indication of the true relation from a rational Theory—the inquiry is too uncertain to be worth pursuing.

12. *Ratios* $U \div v_0$, $U \div v_{\frac{1}{2}H}$.—The Surface-velocity and value of the ratio $c = U \div v_0$ are also shown in Tab. 8 (brought forward from Tab. 3, 4), and have been plotted as ordinates in Pl. XLIII, for each of the same 28 Series. The approximation of the Curve of v_0 to a straight line is evidently much closer than that of any of the Trial Curves treated of in the last Article. This shows that U is much more nearly in simple proportion to v_0 than to any of the preceding trial quantities, and leads to the belief that in the present state of science—

> "A much closer approximation to the central mean velocity may be obtained from direct velocity-measurement of even one primary velocity (say the surface- or mid-depth-velocity) than from any known formula in terms of Surface-Fall",...(53).

The ratios $U \div v_0$, and $U \div v_{\frac{1}{2}H}$ are indeed far from constant (*see* Tab. 3, 4): the following Table gives an Abstract of the Ranges of the Depth (H), Mean Velocity (U), and Ratios ($U \div v_0$, $U \div v_{\frac{1}{2}H}$) for each Site, separately for central and non-central verticals.

Soláni Site.	Series.	H.	U.	$U \div v_0$.	$U \div v_{\frac{1}{2}H}$.	Vertical.
Left Aqueduct, ..	1- 4	9·46–5·92	4·11–3·32	·957–·939	·995–·976	Central.
Right Aqueduct, ..	5-17	9·94–5·55	4·61–3·65	1·018–·961	1·016–·978	
" " [L. Aq. closed]	18-20	4·66–3·99	6·40–5·47	·995–·967	1·000–·995	
Embankt. Main Site,	21-28	10·89–6·16	4·27–2·63	·997–·926	1·002–·977	
Right Aqueduct, ..	29-40	9·55–6·96	4·10–2·63	1·128–·961	·988–·961	Various Non-Central.
Embankt. Main Site,	41-46	8·78–2·58	3·29–2·40	·968–·894	1·045–·984	

The cause of variability of these ratios on any one vertical is by no means clear from the data available: the state of the wind no doubt affects the Surface more than the Mean Velocity, and thus affects the ratio $U \div v_0$, but on careful examination of all known causes of variation, the effects seemed so obscure that the investigation did not seem worth publishing.

The Range of the ratios gives an idea of the amount of error likely to be made in computing the Mean Velocity past a vertical (U) from a good Surface or Mid-depth Velocity-Measurement by application of an *assumed* constant ratio. The best value of the ratio to be used for each vertical could of course only be found by direct experiment on each vertical.

13. *Approximation recommended.*—The formulæ ($27a, b$), ($33a, b$) of Art. 9a, b give highly approximate values of the Mean Velocity past a vertical in cases where the Velocity-Curve is nearly parabolic. Of these the Method recommended in Art. 9b of combined velocity-measurements at the surface and at $\frac{2}{3}$-depth is *by far the most convenient* for practical work. The Unsteady Motion of the water, however, makes any such combined velocity-measurements inconvenient and uncertain, (as explained under "Objections to Twin Floats", Ch. IX, 2b); so that on the whole it is probably *better to attempt only such approximation as can be obtained by velocity-measurements with a single Instrument* (or at a single depth).

And of these the use of the Velocity-Rod (as explained in Ch. XV) is *by far the most convenient* when the depth of water is not > about 15′. In greater depths of water the approximations investigated in Art. 9e are recommended, viz., velocity-measurements at $\frac{5}{8}$-depth in general, and at $\frac{1}{10}$-depth near to vertical banks.

[It should be understood that the depth in question is the *proportionate depth on each vertical*, so that the real depth at which the velocity-measurement should be effected may be *different** *for each vertical*].

14. *Results true on the average.*—The Results of this Chapter which depend on the approximately parabolic figure of the Velocity-Curve, viz., Art. 9, *et seq.*, cannot of course be expected to obtain in single trials in consequence of the Unsteady Motion, but they may be accepted with confidence as *highly approximate on the average* of a great number of trials.

* In the Mississippi Report such velocity-measurements are recommended to be made (in large rivers where the depth is tolerably uniform, *see* p. 295 of Report) *at a constant depth*, viz., at $\frac{2}{3}$ the hyd. mean depth right across: this seems hardly justifiable.

CHAPTER XV.

RODS.

Preface.—This Chapter contains a description (Art. 1—7f) of, and experimental discussion (Art. 8—15) of, the use of RODS for measurement of Mean Velocity past a Vertical. The most interesting Articles are Art. 1—5, 8, 8b (after Result (8)), 8c, 8e (after Result (15b′)), 9, 10—15.

1. **Mean Velocity Rods.**—The use of a slender ROD or "Float-Pole" loaded so as to float nearly upright has often been used for rapidly obtaining an approximation to the Mean Velocity past its Length, *i.e.*, to the Mean Velocity past a vertical. Such an Instrument will for shortness be called a ROD.

The utility of this Instrument depends entirely on whether its actual "velocity" is a sufficient approximation to the Mean Velocity throughout its Length. It is so simple, so cheap, and so convenient in use (in currents not more than 15′ deep) that, if the approximation is sufficient, it should *supersede all other* Instruments when the Mean Velocity past a vertical is the primary quantity sought.

Much attention was therefore paid to this point, both by investigating the theory* of the motion, and by direct Experiment.

2. **History of use of Rods.**—The use of loaded RODS for measuring Mean Velocity past a vertical appears to have been introduced by Krayënhoff about 1812. They were used in the following important Experiments :—

Experimenter.	Epoch.	River or Place.	Reference.
Krayënhoff,	1812	Rivers in Holland,	Mississippi Report, pp. 189, 307.
deBuffon, ..	1821	Tiber,	Mississippi Report, pp. 195, 309.
Destrem, ..	1835	Neva,	Mississippi Report, pp. 192, 307.
Francis, J. B.,	1852	Lowell Canals Massachusetts,	Lowell Expts., Art. 176—187.
Various, ..	1847—73	Rhine,	Verslag aan den Koning, pub. at the Hague in 1876, p. 191.

* The Author has not been able to find any published theoretical investigation on this point.

3. Rod-motion, GENERAL THEORY.—The Instrument consists essentially of a slender cylindric ROD, loaded so as to float upright in still water, of uniform section, and uniform physical state of surface throughout its length.

When dropped into a uniform current, it moves at first irregularly, but after a time it acquires a state of relative equilibrium with the fluid, and after that moves with a tolerably uniform velocity, (which would in fact be quite uniform were the motion of the water itself "steady".) This "terminal velocity" is the velocity to be observed, and is the only one requiring discussion: it will for shortness be styled the ROD-VELOCITY, and will be denoted by u.

When in this state of relative equilibrium, some of the fluid strata into which the Rod penetrates will be moving faster than the Rod, and some slower: the former tend to accelerate the Rod both by direct pressure and by lateral friction; and similarly the latter tend to retard the Rod. And in this state it is clear that the Total Forces of Acceleration (say F) and Retardation (say R) must be *equal and opposite*, so that they form a statical Couple whose effect will be to rotate the Rod into an inclined position until the arm of the contrary Couple—consisting of the weight of the Rod (say W) and the equal opposite Resultant upward Fluid Pressure (say P)—is sufficient to produce a contrary "moment of stability" just equal to the "moment of instability". This is expressed by the equations—

$$F + R = 0, \quad W + P = 0, \quad F \cdot q = P \cdot p \sin\theta, \ldots\ldots\ldots\ldots\ldots\ldots(1),$$

where q = distances between "centres" of F, R,

p = distances between centres of gravity and of buoyancy,

θ = inclination of Rod to vertical.

These are the complete set of "conditions of relative equilibrium" expressed in a general form. The first is the one which determines the Rod-velocity, and will form therefore the basis of future discussion (Ch. XVI): the second is of no interest, and the third serves only to calculate the inclination (θ) of the Rod, which is not of much interest.

3a. *Rod-velocity approximates to Mean.*—It is clear (from the above) that the Rod will finally move with a velocity which is *some sort of mean* of the velocities of the fluid strata into which it dips. The important question is whether this Rod-velocity (u) is *approximately the same* as the proper mean of those velocities. This will be discussed both experimentally (in this Chapter) and theoretically (in next Chapter). But

it may be observed at once that in actual streams the Discrepancy cannot be very great, because the difference between the greatest and least Average Velocities past the same vertical (under same conditions of course) is itself small, (*see* Ch. X, 8:) so that it is obvious *à priori* that the Rod-velocity is at any rate *a rough approximation* to the Mean Velocity past a vertical.

4. *Essentials of a Rod.*—The following are the *Special Conditions to* be fulfilled in a good Mean Velocity Rod, in addition to the General Conditions (Ch. IV, 6) common to every Float, a few of which are here repeated in the special forms they take with this Instrument:—

1°. "The Rod should be a slender cylinder of uniform thickness throughout, as thin as is compatible with the requisite stiffness".

2°. "Its convex surface should be of same physical state throughout, the smoother the better".

3°. "Its centre of gravity should be as low as possible in the water".

4°. "The part exposed to wind should be the least possible consistent with serving its function as a marker",

5°. "and yet should be as buoyant as possible, to secure quick rising to the surface after accidental submergence".

6°. "The loading at foot should be so arranged as to remain at the foot even if the Rod be inverted".

Of the above Conditions the first four are essential to the accuracy of the Instrument, and the two last are practical conditions essential to its convenient use. Thus of the first four—

1° & 2°. Thinness is essential to prevent undue disturbance of the natural motion of the water: the thinner and smoother the Rod the less it disturbs the water. Again, uniformity of thickness and of state of surface are required that the action of the current may be similar throughout the length.

3°. The depression of the centre of gravity is necessary to enable the Rod to float always nearly upright in spite of the varying pressure at different parts of its length. A nearly vertical* position is essential to accuracy, in order that the Rod may be always immersed—when in a state of relative equilibrium with the fluid—to a nearly constant depth.

5. **Length of Rod.**—The term Length of Rod will be generally used for shortness to denote the *immersed length*, and will be denoted by the symbol l: the total length from head to foot will be distinguished as the Full Length.

The two Conditions Nos. 3° and 4° both involve that—

"The Full Length of a Rod should only slightly exceed its immersed Length",..(2a).

* Strict verticality is obviously impossible unless the two parallel Resultant Forces of Acceleration and Retardation (Art. 3) are *directly opposed* to one another, (an exceptional condition).

[For a Rod whose Full Length greatly exceeded its immersed Length would obviously violate Condition 4°, but it would also violate Condition 3°, for it would be impossible to load it sufficiently at the foot to depress its centre of gravity deeply in the water, and it would consequently often float in a very oblique position].

It follows therefore that—

"A long Rod is quite unfit for use with small immersion", ...(2b).

[The excess of Full Length over immersed Length adopted in these Experiments was 3", 2", 1", ½" for immersions over 6', between 6' and 3', between 3' and 1', and less than 1' respectively].

5a. Depth of immersion.—It is obvious that to prevent the Rods touching the bed—

"The vertical depth of immersion is always necessarily < the full depth (H)", ...(3a),

also that, in consequence of the Rods floating, seldom vertically but, usually in a slightly inclined position—

"The vertical depth of immersion is usually *slightly* < the immersed Length (l)", ...(3b).

As the real depth of immersion is unknown *à priori*, and would be difficult to observe, it must be assumed in what follows as nearly equal to (but generally <) the immersed Length (l). It follows also that—

"The immersed Length (l) is usually < the full depth (H)", ...(3c).

[But it happened on certain rare occasions (*see* Ser. 30, 37, 39) that the immersed length (l) slightly exceeded the full depth (H). The possibility of such a case is of course due to one or both of the following causes:—

1°. The tilt of the Rod lifting its foot sufficiently to clear the bed.

2°. Lightness (*i.e.*, imperfect adjustment) of the Rod, whereby its real immersion in still water would be less than its intended immersion,

but the excess was in every case unimportant, the maximum being ·15 of a foot].

6. SET OF RODS.—The Conditions ($2a, b$), ($3a, b, c$) of Art. 5, 5a involve the provision of a set of Rods of various lengths *suited to various depths of immersion*. Again, the Unsteady Motion of the water necessitates the provision of a *considerable stock of each particular length* (Ch. VI, 7) to enable Field-work to be done with any convenience.

[The Rods were made up for these Experiments of following Lengths:—

Under 1', of ·1, ·2, ·3, &c., advancing by tenths up to 1'.

1', 2', 3', and so on by one-foot increments up to 12', in 1875—1876.

1', 1½', 2', 2½', and so on by half-foot increments up to 12', after 1876.

The complete Set of Rods for one Field-party at the larger Sites consisted of 12 of each Length in most frequent use (7' to 9½'), and 6 of all other lengths required. A complete Set of Rods is therefore a pretty bulky and heavy mass: thus a single Set of Rods of only one of each length amounts to about 154 running feet of Rod, and a "complete Set" as above amounts to about 1230 running feet. The bulk, weight, and cost of such a Set of Apparatus becomes a serious matter, and on all three of these scores (besides the essential of accuracy) the use of the *thinnest* Rod consistent with sufficient strength and stiffness becomes a practical necessity.

[The standard thickness adopted in these Experiments was *one inch*].

7. Description of Rods, (Pl. XXIV, 5—9).—Three different patterns of Rod were used at different times; these will be termed for shortness—

1″ *wood Rods*, used in 1875 and till May 1876.
" " used also in short lengths (under 1′) throughout the work.
$2\frac{1}{8}$″ *wood Rods*, used in March and April 1875, (*see* Ser. 102, 104.)
1″ *tin Tube-Rods*, used from Febry. 1876.

Here follows a detailed description of these Rods, (Art. 7a—f)—

7a. 1″ WOOD RODS, (Pl. XXIV, 5).—A slender cylindric Rod (*Ll*) of 1″ diameter, made usually of a moderately heavy wood (as "Sál," *Shorea Robusta*; or "Tún", *Cedrela Túna*), and sometimes of a light wood (as "Deodár", *Cedrus Deodara*) was loaded at one end *l* with small cylindric weights (w) of the same diameter as the Rod. The weights consisted of discs of sheet tin, and of small cylindric pieces of lead (w) of 1, 2, 4, 8 tolas* weight respectively: they were fixed at the foot *l* of the Rod by a long thin screw (*l*) passing right through them all. The Rods were loaded by experiment in still water until immersed (including the length of the weights) to the desired depth, leaving from 1″ to 3″ projecting above the surface. A small screw (*s*) was slightly screwed on to the head of the Rod to be used for attaching a small pledget of cotton wool to serve as a "marker".

To decrease the absorption of water by the wood, the Rods were coated sometimes with paint, sometimes with oil.

Objections. The chief objections to this pattern arise from the use of wood, and from the mode of fixing the loading.

1°. If a light wood be used, the amount of lead required to sink the Rod when over 6′ long forms a cylinder of such length as to be difficult to fasten on securely by a simple screw.
2°. If a heavy wood be used, the available buoyancy is not enough to carry a "Load" at the foot sufficient to depress the centre of gravity deeply.
3°. It is extremely difficult to procure wood which will yield straight Rods of 10′ length (and upwards) of only 1″ diameter, free from knots and flaws.
4°. Wood is so hygroscopic, that the fine adjustment required is rapidly destroyed by immersion in water.
5°. The mode of fixing the lead weights—viz., by a long screw passing through them—is insecure. The weights are liable to be detached whilst in use.
6°. If the screw used for attaching the weights breaks off short, the Rod cannot be repaired, but can only be converted into a shorter one by cutting off the foot.
7°. The lead weights are liable to be stolen.
8°. These Rods proved expensive in lengths exceeding 6′, partly from the good quality of wood required (*see* 3°), and partly from the quantity of lead required for the loading.

These objections were found to be so great, that the use of this pattern was entirely given up in 1876 for all lengths exceeding one foot.

* One "tola" = 180 grains.

7b. $2\frac{1}{2}''$ WOOD RODS, (Pl. XXIV, 6).—A few RODS of a decidedly superior finish, made up in the "Roorkee Workshops" for the use of the Ganges Canal Staff, were lent for use on these Experiments from the Northern Division Ganges Canal Office.

These were neatly made cylindric wooden Rods of $2\frac{1}{2}''$ diameter, fitted with loaded iron tubes at the foot *l* of same diameter as the Rod, into which bullets, shot, &c., could be introduced (for adjusting the depth of immersion) through a small hole (*a*) closed by a sliding shutter: they were neatly painted all over, and marked in feet, and quarter-feet. Four claw hooks were fitted on the head to enable the Rod to be easily caught. They were made up in Sets of only three (one 8′, one 9′, one 12′ long).

Objections. The chief objection to this pattern is its expense, the Set of three costing about thirty rupees (*i.e.*, about £3), so that they could not be made up in large numbers. The Set of only three lengths is obviously useless for *general use*, (the 9′ length for instance is useless in water between $3\frac{1}{2}'$ and 8′ deep,) *see* Art. 5.

There are also minor objections. The thickness $2\frac{1}{2}''$ is probably too great for accuracy, and it involves the 12′ length being inconveniently heavy for handling. The sliding shutter was liable to be broken and lost, upon which the bullets, &c., frequently fell out and were lost.

This pattern was used only in the two Mean Velocity Ser. Nos. 102, 104 done in 1875 in the Left Solání Aqueduct.

7c. 1″ TIN TUBE-RODS, (Pl. XXIV, 7).—This pattern was a "Tube" of 1″ bore made of sheet tin, in lengths corresponding to the width of the sheets obtainable. The longitudinal joints of alternate lengths were arranged on opposite sides of the Tube so as to break joint. The joints between the lengths were "butt joints" formed over a short "joint-piece", or similar tube 3″ long, fitting easily inside the main tubes, ($1\frac{1}{2}''$ inside each.) A piece of 1″ rod-iron (w) was fixed inside the lower end (*l*) of the Tube, of such length as to sink the Tube in an upright position in still water nearly to the desired depth, and formed in fact the chief part of the "loading". The fine adjustment to the required depth of immersion was done by dropping small bits of iron, and lastly shot into the Tube in still water. Finally the head of the Tube was sealed with a tin disc, (sometimes flat, sometimes slightly rounded.) The outside was then coated with a coat of cheap black paint to protect it from the water. The "water line" (L) on each finished Tube-Rod was marked by a ring of red paint, and the LENGTH (Art. 5) of the Rod was figured on it in red paint to enable each length to be readily recognised.

The Rods made up after Sepr. '78 were fitted with an arrangement for confining the shot, &c., used for the fine adjustment to the lower end of the Tube: this was simply a sort of conical funnel (*f*) also of sheet tin fixed inside the Tube just above the iron foot; this funnel allowed the shot, &c., to pass downwards when the Tube was held erect, but rendered their return difficult when the Rod was laid flat or even inverted.

These Rods were made up in large numbers for this work by a common native tinsmith in the bazar, at the rate of about $2\frac{1}{2}$ annas a foot, and required only testing on delivery.

Plank-Trays, and Handling, (Pl. XXIV, 8).—The longer Tube-Rods were of course somewhat fragile, and were liable to be strained if unsupported when in a horizontal position. When not in actual use they were accordingly laid out on 1″ planks with strips of wood screwed on at the ends and sides so as to form a sort of

PLANK-TRAY (Pl. XXIV, 8) about 9" wide with raised edges about 3" high which served to prevent the Rods slipping off. Each Plank-Tray held about 18 Rods. The Rods being of same thickness throughout lay flat on the Trays without being strained. The Rods were usually lifted and carried about on the Plank-Trays: if required to be lifted and carried without the Trays, the longer Rods were always carried in an upright position with the loaded ends down.

[By moderately careful handling the Rods lasted a considerable time: of course after a time the joints became strained and admitted water into the Tube. This defect caused the Rod to sink deeper than usual in the water, and was therefore easily detected during use; any Rod in this condition was at once withdrawn from use, and sent for repair].

Advantages of Tube-Rods. The Tube-Rods proved by far the best of the patterns tried: they fulfil *all* the six Conditions of Art. 4 admirably. They are strong enough and stiff enough to bear a good deal of use; they are light, easily packed, convenient in use, easily repaired, pretty cheap, and not worth stealing.

7d. ADJUSTMENT and TESTING.—The adjustment of the "loading" of the Rods to proper depth of immersion was always done in still water. Similarly all Rods received from Contractors were tested in still water before being taken into use. An error not exceeding $\frac{1}{4}''$ in the adjustment of Rods not less than one foot long was considered admissible.

The most convenient mode of testing is to simply place the Rods in a still water pool or tank; but as a still water pool of sufficient depth (over 12′ required) is not always readily accessible, it is worth noting that the adjustment and testing can be quite conveniently done inside a long tube of a few inches diameter standing upright.

[The Test-Tube used in these Experiments was (*see* Pl. XXIV, 9) simply a pipe of sheet tin 14′ long by $4\frac{1}{2}''$ diameter placed in an upright position against the side of a verandah, so that the top of the pipe was accessible for use from the verandah roof. The top of the pipe was provided with a loosely fitting cap to exclude dust when not in use, and the foot was provided with a stop cock (s) to let out water. This arrangement proved very convenient].

7e. *Loss of Rods.*—There was a constant small loss of Rods during Field-work from their sinking outright. Both kinds of Rod, the wood and the tin were liable to become too heavy to float during actual use; the wooden ones from absorption of water, and the tin ones from leakage of water. But by far the most frequent cause of sinking was from the foot of the Rod being caught by something on the bed (*e.g.*, on snags, brick or clay heaps, or other projections); unless immediately released the current-pressure carried the Rod under, and *laid it flat on the bed,* where it usually lay (without being swept down-stream by the current) until released. A very slight touch, enough just to disturb the Rod, sufficed to bring it to the surface again.

[Rods so sunk were constantly recovered by "fishing" for them with a pole with a hook at the end, or, if this failed, by diving for them, just about the spot where they sank].

7f. *Top-hooks useless.*—It was suggested to the Author that the provision of "claw-hooks" at the top of the Rods (as in Pl. XXIV, 6) would allow of the Rods being caught after passing out of the Run upon an extra Rope strained across the stream for the purpose, and so render the use of the Lower Boat (always used in these Experiments for catching the Rods) unnecessary.

If this were a practicable and fairly handy arrangement, it would often be a great convenience in the use of these Rods by the regular Canal Staff, as it is not always possible to provide two Boats suitable; the expense of the men employed in the second boat would also be saved.

On account of the practical importance of this question, it was deemed right to give the arrangement a fair trial. It was accordingly tried both at the Belra Site (190′ wide) and at the Kamhera Site (70′ wide): every possible attempt was made to secure success, but the arrangement was found nearly impracticable with any sort of convenience.

The practical difficulties were found to be of two kinds—

1°. The uncertainty of the Rods hitching properly on the Rope.

2°. The difficulty of bringing them to bank when hitched.

It seems unnecessary to enter into these difficulties in detail. Suffice it to say that from one cause or another, a large proportion of the Rods cast from the Upper Boat could not be successfully brought to bank by this process. When once slipped, the only way to catch these Rods (in actual practice) would then be either to send the Upper Boat (supposed to be the only one in use) after them, or else to send a man in swimming after them. Either mode would be very inconvenient. The detaching of the Upper Boat for this purpose would sometimes involve the loss of half an hour, or even more, (a serious matter under an Indian sun.) This amounts to saying that the use of a Lower Boat for catching the Rods is indispensable to practical convenience, so that the addition of the hooks does not secure the advantages sought.

On the other hand the presence of the hooks has inconveniences of its own: the spread of the hooks—being necessarily greater than the thickness of the Rod—prevents them lying flat on one another. This involves a constant transverse strain on them (which they are very ill able to bear), and also the occupying more space in packing.

8. Rod-motion, Experiments.—A very extensive course of Experiment was undertaken to compare the Rod-velocity (u) with the Mean Velocity past a vertical (U). This was tried in two distinct ways—

i. By comparing the Mean Velocities (U_z) through various depths (z) less than the full depth (H) with the Rod velocities (u_z) of Rods of Lengths (l) the same as those depths (z), (*i.e.*, $l = z$).

ii. By comparing the Mean Velocity (U) through the full depth (H) with the Rod-velocity (u) of a Rod of Length (l) nearly equal to H.

The latter is the more important in a practical view, as the chief use of the Rods in actual practice would be for measurement of the Mean Velocity through the *full depth* (H) on each vertical, and not through a part only of it (z). The course of Experiments for the latter (No. ii) was accordingly much the more extensive. The former research, however, is equally important scientifically as the latter.

8a. Comparison i, FIELD-WORK.—The three SERIES, Nos. 3, 4, 12 (of Subsurface work) were executed specially in such a way as to yield the requisite data viz., by running Rods and Double-Floats of same nominal depth of immersion (*i.e.*,

$l = z$) in pairs together (so that each pair should be passing through the RUN close together).

[The order was—Three Surface-Floats, followed by—

One 1' Double-Float and one 1' Rod, three times in succession;

One 2' Double-Float and one 2' Rod, three times in succession;

and so on down to the lowest depth (n feet) attainable].

It will be seen that this furnished the data required: using notation indicated in following scheme:—

Depth (in feet) from surface, $z = 1, 2, 3, \ldots (n-1), n, \ldots H$=full depth.
Discharge (past the depth z), $D_z = D_1, D_2, D_3, \ldots D_{n-1}, D_n, \ldots D_H$, or D.
Mean-Velocity (through the depth z), $U_z = U_1, U_2, U_3, \ldots U_{n-1}, U_n, \ldots U_H$, or U.
Rod-Velocity (of Rod of length z), $u_z = u_1, u_2, u_3 \ldots u_{n-1}, u_n, \ldots u$

The superficial Discharges (D_z) past the several lengths z (= 1', 2', 3', ... $(n-1)'$, n feet) of the vertical of full depth H were calculated with the formulæ given in Ch. XIII, 2b from the velocities given by the Double-Floats: the Mean Velocities (U_z) through each of these depths (z) were found (as in Ch. XIV, 1) by the formula

$$U_z = D_z \div z, \text{ for every value of } z, \ldots\ldots\ldots\ldots (4).$$

The Rod-velocities (each deduced from mean of timings of 3 Rods) were formed into SETS and SERIES, and tabulated precisely in the same way as the Subsurface Velocities of these SERIES (3, 4, 12), forming therefore corresponding pairs of SERIES made up of pairs of corresponding SETS *executed to the smallest details under the same External Conditions*, as nearly as is possible in this sort of Experiment.

The details of these SERIES of Rod-velocities have been given as Series Nos. 10R, 11R, 12R at pp. 93, 94 of the 1874-5 Report. But as it is only proposed to compare the Average values of U_z, u_z of a whole Series, (and not the values thereof for each Set separately), it seems sufficient to republish only these Average values; these alone are now given in Abstr. Tab. 9.

8b. *Discussion of* i.—A glance at the vertical curves given by the above (Pl. XII, XIII, Ser. 3, 4, 12) will show that in all three the maximum velocity line is depressed below the surface, from which it arises that—as may also be seen from Tab. 9—

"The Mean Velocity-Measurements (U_z') from the Double-Float are at first greater than the surface velocity for a short depth below the surface, and then less than the surface velocity, and decreasing steadily downwards towards the bed", (5).

Now it will be seen from Ch. XIV, 6b that the error of the Mean Velocity measurement (U_z') deduced from the Double-Float is generally as follows:—

$$U_z >< U_z' \text{ when } v_0 <> U_z', \ldots\ldots\ldots\ldots (6),$$

so that *in such a case as that of the Series in question*—

"The differences ($U_z - U_z'$) are at first +, then −, and increasing with the depth below the surface", ... (7).

But the Table quoted (No. 9) shows that, in these Series—

"The differences ($u_z - U_z'$) are at first +, then −, and increasing with the depth below the surface", ... (8).

Comparing the last two Results it is seen that—

"The Rod-velocity (u_z) exceeds and falls short of the Double-Float Mean Velocity (U_z') in those regions where the latter is known to be short of, or in excess of,

the true Mean Velocity (U_z), and the latter Difference ($u_z - U_z'$) increases towards the bed, much as the excess error of the Double-Float Mean Velocity does ",...(9a).

From this it follows that—

"The Rod-velocity (u_z) is *probably a closer approximation* to the Mean Velocity (U_z) through the depth z than the value (U_z') given by the Double-Float, especially when that depth (z) is a large fraction of the full depth (H), in which case the Double-Float is used at a disadvantage",.. ....(9b).

Now the determination of the Mean Velocity through the full depth (H) is a far more important practical question than its determination through any portion (z) of that depth, and it is seen that the use of the Double-Float is then the most disadvantageous. This is obviously all in favor of the use of the Rod for measurement of Mean Velocity past a vertical.

8c. Comparison ii.—The preceding Experiments having given some confidence in the use of the Rods for measurement of Mean Velocity (U_z) through all depths (z) less than the full depth (H), a far more extensive Series of Experiments was undertaken for testing their use for measurement of the (practically far more important quantity) Mean Velocity (U) through the full depth (H).

To effect this it was proposed to compare the value of Mean Velocity past a vertical deduced from the use of the Double-Float (as in Ch. XIV, 5) with the Rod-velocity of Rods sunk nearly to full depth.

8d. FIELD-WORK, TABULATION.—In executing each SET of Subsurface work with Double-Floats at the 1', 2', 3', &c.,......n feet (nominal) depths as detailed in Ch. X, 5 the general practice* was to obtain six measurements of Rod-velocities after or at the close of each SET, (Ch. X, 5,) using always Rods of "Length" (l) nearly equal to the full depth (H).

[The Rods being made up only in the lengths detailed in Art. 6, it resulted that, l being the "immersed length",

$$\left.\begin{aligned} l &= n \text{ feet usually when } n \text{ is nearly equal to (but} < \text{) H,} \\ &= (n+\tfrac{1}{2}) \text{ feet occasionally when } n \text{ is several inches less than H,} \\ &= (n-\tfrac{1}{2}) \text{ feet occasionally when } n > \text{H, (\textit{see} Ch. X, 13, i),} \end{aligned}\right\} \cdots\cdots\cdots(10),$$

where n = length of longest Connector in feet.

The "velocity" deduced (Ch. IV, 34a) from the mean of the timings of the six Rods was accepted as the proper measure of the ROD-VELOCITY (u) for comparison with the MEAN VELOCITY† (U) past the vertical deduced (as explained in Ch. XIV, 5) from the particular SET of Double-Float velocity-measurements which it accompanied, the Field-work for deducing both *having been executed in concert under nearly the same External Conditions.*

The two quantities (U, u) are entered together in Col. 8, and the "Length" of Rod used in Col. 2 of all the Tables VII—XXVIII, for every Set of Subsurface work, (except in Ser. 2.) To facilitate the comparison, the difference (u − U) also is taken out for every SET in Col. 9. Lastly, the RANGE, (*i. e.*, difference between

* carried out in every Series except No. 2.

† In what follows, as well in Tab. VII—XXVIII and 3, 4, the use of the accented U has been dropped for the Mean Velocity-measurement given by the Double-Float.

greatest and least,) and AVERAGE of these velocities (u, U) and differences (u − U) in each SERIES are entered in the lines δ, v respectively at foot of each SERIES.

An Abstract of these Results is also given in Abstr. Tab. 3, 4, viz., the Average Mean Velocity (U), Average Rod-Velocity (u), and the difference between them (u − U), and also the Range of both U, u for each of the 46 Series.

8e. *Discussion of* ii.—It will be sufficient to compare the Average values of U, u in the Abstr. Tab. 3, 4. It was shown in Ch. XIV, 6b that, on the whole, the Average value of U might be expected to be in error as follows :—

"U is too great, when the max. velocy. line is high (Z not > about $\frac{1}{3}$H)",(11a).

"U is too small, when the max. velocy. line is low (Z not < about $\frac{1}{3}$H)", (11b),

or again—

"U is too great, when < v_0",..(12a).

"U is too small, when > v_0",..(12b).

Now if u be a closer approximation to the true Mean Velocity than U, then

"u should be < U in Cases (11a), (12a) ",...(13a).

"u should be > U in Cases (11b), (12b) ",...(13b).

[Of course no inferences can be drawn with certainty in cases approaching the critical limits, *i.e.*, when Z = ·333 H *nearly*, or (U − v_0) is a very *small* quantity].

Now on examining the three columns of (u − U), Z ÷ H, (U − v_0) in Abstr. Tab. 3, 4 ; it will be seen that—

"(u − U) is negative, (*i.e.*, u < U), in 31 out of the Total of 33 cases in which Z ÷ H < + ·34, (including cases when Z is negative) ",..........................(14a).

[The exceptions are only Ser. 4, in which u = U, and Ser. 20, in which (u − U) = + ·03 : the Rods used in this last were only 3′ long in a depth of 3′·99 ; had longer Rods been used, the value of u would certainly have been smaller, and possibly < U].

"(u − U) is positive, (*i.e.*, u > U) in 6 out of the Total of 8 cases in which Z ÷ H > + ·34 ",..(14b).

[The 2 exceptions are Ser. 34, in which Z ÷ H = ·392, and u = U,
and Ser. 38, in which Z ÷ H = ·394, and (u − U) = − ·10].

Also, " (u − U) is negative, (*i.e.*, u < U), in 32 out of the 33 cases in which U < v_0". [The exception is Ser. 4, in which (u = U)],..............................(15a).

"(u − U) is positive, (*i.e.*, u > U) in 6 out of the Total of 7 cases in which (U − v_0) > ·10 ",...(15b).

[The exception is Ser. 34, in which (U − v_0) = + ·16, and u = U],

also, "(u − U) is negative, *i.e.*, u < U in all the 5 cases in which (U − v_0) is only a *small* + quantity not > ·10 ",.. (15b').

Thus on the whole the difference (u − U) between the Rod-velocity and Mean Velocity-measurement is of that character which might be expected (*see* Results (14a, b) & (15a, b, b') above) from the known error of the latter.

It will be seen that all this is strongly in favor of the use of the Rods, and points to the important Conclusion—

"The Rod-velocity of a Rod whose vertical depth of immersion is nearly equal to the full depth (H) gives an approximation to the real Mean Velocity through the full depth (H) of that vertical *generally closer than that obtainable by the use of the Double-Float* ",..(16).

9. Steadiness of Rod-motion.—It has been explained (Ch. VI, 17) that there is a constant *lateral interlacing* of the stream-lines in consequence of which FLOATS (of all kinds) move in a tortuous path, edging sometimes to the right, sometimes to the left. This irregularity of the Float-path is far less in the case of the RODS than of any small FLOATS, (whether Surface-Floats or Double-Floats,) as might be expected, because many of the transverse pressures due to fluid particles moving to right or left must necessarily balance in the case of a ROD, and the actual "Deviation" of the Rod to right or left is only that due to the unbalanced portion of the lateral pressures.

The principal source of the tediousness of velocity-measurement with Floats, viz., the irregularity of the Float-paths, is thus much reduced in the case of the Rods.

This is well shown in the following special Experiment on this point:—

9a. *Experiment.*—A succession of 100 8" Surface-Floats and 100 1" tin Tube-Rods of 9' length were run *in as rapid succession as possible*, (Surface-Float and Rod alternately,) down the mid-channel line of the Solání Embankment Main Site. Every FLOAT was timed in the usual way through the 50' Run, *whether in fair course or not*, and its "Deviation" to right or left of the centre in passing under the Lower Rope recorded, with the object of comparing the maximum Deviation ordinarily possible with the two Instruments. The Results were (*see* Tab. LXXIV)—

Surface-Floats. Max. deviation, to left 12'; to right, 11'; Total 23';
Rods. Max. deviation, to left 5'; to right, 5'; Total 10';

showing a far larger deviation with the Surface-Floats than with the Rods.

Again, when in use near the edge, the Rods do not partake (or at least not sensibly) of the constant *set* of the surface-water away from the edge, (explained hereafter, Ch. XVII, 14a): this enables them to be used closer to the banks, and also with longer Runs when near the banks than is possible with any other sort of FLOAT.

The two principal sources of the tediousness of velocity-measurement with Floats, viz.—

1°. Irregularity of the Float-path at all parts of the channel,

2°. Set of the surface-water near the edge towards the centre,

are thus much reduced in the case of the Rods. This is a most important *practical* advantage, as it makes velocity-measurements with Rods easier, and—on the whole—quicker of execution than with any other sort of FLOAT, (even though the Rods move actually the slower,) because the proportion of Rods that run in "fair course" is much greater out of the whole number cast into the water than with any other sort of FLOAT.

Again, the variability of the Rod-velocity is markedly less than that of the surface- and subsurface-velocities. This may be seen by examining the "Ranges" given for all the velocities at foot of each Series in Tab. VII—XXVIII, or more rapidly (for the case of v_O, v_{1H}, v_H, u only)

2 H

by comparing the "Ranges" given in Abstr. Tab. 3, 4. An Abstract of the Total number of Series in which the Range of u is less than, and not less than, the Ranges of v_0, $v_{\frac{1}{2}H}$ is given in following Table:—

	Comparison of Ranges.	Solani Sites. Central Vertical. L. Aqued.	Central Vertical. R. Aqued.	Central Vertical. R. Aqued. (L. Aq. closed).	Central Vertical. Embankt. Main Site.	Non-Central Vertical. R. Aqued.	Non-Central Vertical. Embankt. Main Site.	Total Number of Series.
Total Number of Series.	Range of u < Range of v_0, ...	0	11	3	6	9	6	35
	Range of u = or > Range of v_0, ...	3	2	0	2	3	0	10
	Range of u < Range of $v_{\frac{1}{2}H}$, ...	0	10	2	4	7	3	26
	Range of u = or > Range of $v_{\frac{1}{2}H}$, ...	3	3	1	4	5	3	19

The general Conclusion from the above, and indeed from the (very extensive) experience gained in the whole course of the Experiments is that—

"Rod-motion is decidedly steadier (as regards both irregularity of Float-path and variability of velocity) than the motion of small Floats",....................(17).

10. **Advantages of Rods.**—Admitting that Rods of immersed length (l) nearly equal to the full depth (H) do give a fair approximation to the Mean Velocity (U) past the vertical, the advantages of their use for that purpose—as compared with the Double-Float—are very great in the case of streams less than about 15′ deep.

The theoretical advantages are—

"The Rod is free from the uncertainty attending the Instability and (unknown) Lift of the Sub-Float",..(18).

"The approximation to the Mean Velocity appears to be commonly *actually closer* than that given by the Double-Float",..(19).

The practical advantages are—

"The Result is obtained more rapidly",..(20).

"The Instrument is more easily handled, and less delicate",....................(21).

"It is simpler in construction, cheaper, and more durable",....................(22).

As the only other known process—not involving the use of the Double-Float—of obtaining the same result (Mean Velocity past a vertical) with moderate rapidity *by direct Experiment*, is that known as "Integration" with a Current-Meter, a somewhat difficult process, attended with uncertainties of its own in addition to those inherent in the ordinary use of the Current-Meter, the above advantages of the Rods seem to justify the Conclusion that—

"For measurement of Mean Velocity past a vertical, the RODS should supersede all other Instruments in cases favorable to their use",............................(23).

The practical importance of this Conclusion is very great indeed.

11. Conditions favorable to Rods.—In addition to the Conditions favorable for the use of FLOATS (Ch. IV, 9) of any sort, viz.—

"A Reach of nearly uniform Cross-section and average bed-slope throughout a great length",..(24),

it is also necessary for the favorable use of Rods that—

"The Bed should be tolerably even, lengthways",...................................(25).

"The depth should not exceed about 15'",..(26).

11a. *Condition* (25).—This is necessary to accuracy. It is clear, that—in order that the Rods may run "free"—*i.e.*, without touching the bed—

"In each Float-course the immersed Length (l) of Rod must be < the *minimum* depth along the Float-course",...(27).

Thus the presence of a single *isolated* "bar" across the bed or across even part of the bed just above or within the RUN would be very unfavorable, as it would involve the use of an unduly short Rod, which would move with a velocity higher (and therefore presumably greater than the Mean Velocity sought) than that of a Rod long enough to reach nearly to the bed elsewhere. It would seem therefore that—

"Roughness of bed, and especially the presence of isolated bars just above or within the Run, causes over-estimation of the Mean Velocity past a vertical",(28).

11b. *Condition* (26).—This is simply a practical limit—

There is obviously some length beyond which the Rods would be so heavy and unwieldy as to be unmanageable: the longest Rods used in these Experiments were 11½' in length in about 12' of water. No great difficulty was found in handling these in a current of about 5 feet per second, and it seemed to the Author that there would be no great difficulty up to about* 15' length.

[It seems possible, however, that the essentials of the Rods above described (viz., those of uniform thickness and uniform surface throughout the length) might be secured for greater depths by the use of some flexible material admitting of coiling like a rope. It should be specifically lighter than water so as to admit of *heavy loading* at the lower end: the tension produced would probably suffice to keep it nearly straight].

12. Preparation of Site.—From what precedes, it appears that a Site where Rods are to be much used should be prepared by "dressing" the Bed and Banks as follows:—

"The bed should be dressed for a length greatly exceeding the whole Rod-path (say not < 250') to a tolerably uniform cross-section and longitudinal slope, (viz., the average cross-section and bed-slope of the locality)",........................(29*a*),

and,—

"The Banks should be dressed to a tolerably uniform slope (viz., the average slope of the locality) for an equal length (250'); and, if likely to suffer erosion, should be protected throughout this length by revetting with masonry",......(29*b*).

* For the mode of Measurement of Mean Velocity past a vertical recommended for greater depths, see Ch. XIV, 13.

[The dressing the banks to a uniform slope is essential to use of the Rods with any convenience near the Banks and actually over the Side-Slopes; any roughness in the Banks interferes most annoyingly with the use of the Rods (near the banks). The masonry lining need not be expensive: brick on edge laid dry will do very well, (*see* Ch. III, 14, 15)].

13. Discharge past a vertical.—The approximate Mean Velocity (u) past the vertical of full depth (H) being known, it follows that, (to the same degree of approximation,)—

Discharge past the vertical (of depth H), or $D = u \, . \, H$,........................(30).

With a bed on a uniform slope, the depth (H) is of course constant along a Float-course: but with a rough bed, the depth along the Float-course varies, and it is not at once obvious what quantity should be understood for H in the above expression. On the whole the AVERAGE DEPTH along a Float-course appears to be the best quantity to use for H, partly for reasons given in Ch. V, 13, and partly because the Conclusion (16) that the Rod-velocity is a closer approximation to the true Mean Velocity than the value (U) given by the Double-Float is *based upon values of* U *computed for the Average Depth.*

For these reasons the AVERAGE DEPTH along a Float-course, (obtained as in Ch. V, 13—17) has been used for H in above formula throughout these Experiments.

[It has been suggested to the Author that the *minimum depth* along a Float-course is the proper quantity to use for H in the above expression. This might be true if the water were *still* (*i.e.*, not moving forward) in the hollows below the level of the ridge in the bed which defines the minimum depth in question. But there is reason to believe that the water is in motion in these hollows: in fact in clear streams with a sandy bed, particles of silt can often be seen moving gently along over the bed, and indeed the mere existence of the hollows points to scour, *i.e.*, motion of the water along the bed and in the hollows themselves (*see* Ch. III, 4). Thus the *minimum depth* on a Float-course does not seem to be the proper quantity to use for H in above expression].

14. Instrumental Errors.—The only obvious possible sources of error inherent in the Instrument seem to be—

i. *Tilt of Rod.* ii. *Shortness of Rod.*

14a. TILT OF ROD.—The cause of the "Tilt" (or obliquity of position) of the Rod has been explained in Art. 3, 4. The practical effect of this Tilt is to make the vertical depth of immersion (and therefore the "effective length") of the Rod somewhat less than its immersed length. The Error caused is therefore similar to that due to use of a short Rod which will be considered under the next head.

With well designed Rods, in which the centre of gravity is low down, the Tilt is always small, so that the error due to this cause is quite trifling.

14b. *Shortness of Rod.*—It is clear that the Rod—whose vertical depth of immersion (l) is necessarily *less* than the full depth (H)—not penetrating into the lowest fluid strata, is not directly affected by these lowest (and therefore slowest moving) strata. It would seem therefore probable *at first sight* that the Rod-velocity (u) is always necessarily greater than the real Mean Velocity (U). This, however, by no means necessarily follows, because the Rod-velocity (u_z) of a Rod of length ($l = z$) is not *really equal to* the Mean Velocity (U_z) throughout the depth ($z = l$) of immersion of the Rod, but *only nearly equal*, so that it is quite possible there might be a compensation of errors otherwise apparently due to this cause.

The Experiments above discussed do not indeed actually *prove* this, but they leave it *probable*, because they show (*see* Art. 8b, 8e) that, denoting the value of the Mean Velocity *through full depth* (H) given by the Double-Float by U',

$u <$ U' in cases where U' is known to be too great,..............................(31*a*).

$u >$ U' in cases where U' is known to be too small,..............................(31*b*).

It is of course quite possible that the value of u might be actually too great *in both cases;* but, if it be admitted that the value U' given by the Double-Float be any tolerable sort of approximation at all, it is much more probable that the value u is not always too great.

In fact all that can be said *à priori* so far is that—

"The Rod-velocity (u) of an *unduly short* Rod is greater than the Mean Velocity (U) past its vertical",..............................(32).

But it will be shown in next Chapter (Ch. XVI, 10), that—

"The Rod-velocity (u_z) of a Rod (whose depth of immersion is $l = z$) is *less than the Mean Velocity* (U_z) *throughout the same depth* (z), z being $<$ H",.......(33*a*),

or in other words that, for the ordinary use of Rods—

"The Mean Velocity (U) past the vertical of the full depth (H) is equal to the Rod-velocity (u) of a Rod whose vertical depth of immersion (l) is somewhat $<$ the full depth (H)",..............................(33*b*),

so that there *is* in fact the compensation of errors spoken of.

This very important Result *disposes of one of the principal objections* hitherto urged against the use of Rods. [For fuller discussion, *see* Ch. XVI, 10, 11].

15. Other Opinions.—These Conclusions (33*a*, *b*) are *contrary to the hitherto general belief:* the present Author has not, however, been able to find any considerable amount of evidence in support of the belief that the Rod-velocity is necessarily greater than the Mean Velocity past the vertical.

15a. LOWELL EXPTS.—In this Work the Conclusion is advanced (Art. 179) that the Rod-velocity *should be* always greater than the Mean Velocity past the same vertical for two reasons—

1°. In consequence of not partaking of the irregular intimate motion of the water.

2°. In consequence of not reaching into the slowest fluid strata over the bed.

The first objection is merely that quoted from Weisbach (*see* Ch. IV, 7f), and already disposed of in the Article quoted.

The Experiments made to test the second point were 11 in number, (Lowell Expts.,

Art. 179—185, and Pl. XI: the dimensions given below are only approximate, being mostly taken by scale from the Plate quoted).

Water was admitted from a Canal about 80′ wide into a narrow lock-channel (at one side of, and parallel to, the main Canal) about 180′ long by 20′ wide, the upper portion of which was of uniform rectangular section for a length of 140′ from the head; the remaining 40′ length was not quite so regular. This 20′ channel terminated in the lock-passage, which was only about 12′ wide, the contraction taking effect wholly from one (the left) bank. The lock-passage contained two lock-chambers, of about 110′ and 90′ length respectively, the upper lock gate being about 25′ below the head of the passage.

Velocity-measurements were made in the upper uniform 140′ length at many different points of its breadth with loaded 2″ tin Tube-Rods nearly reaching the bed, and from these the Total Discharge was computed much in the same way (though with different formulæ) as that used in the present Work, (Ch. XIX, 3, *et seq.*)

This short uniform length within which the Rod-velocities were measured is termed a* FLUME. In the first four Experiments the FLUME was 20′ wide (being the full width of the channel). In the remaining seven Experiments a wooden partition was inserted along the central line of the 140′ channel throughout its whole length, thus dividing it into two chambers each 10′ wide; the right chamber was closed up, and the left chamber used as a FLUME.

The lower lock-chamber was—for these Experiments—enlarged about 25′ from its head into a nearly square basin about 34′×32′, from which the water was discharged through three rectangular openings (or weirs) of about 27′, 24′, 27′ in length in three of its sides. The depth of water passing over the three weirs (in no case exceeding one foot) was found by taking the water level at 4 points inside the basin with "hook-gauges". The Discharge passing over the three weirs was computed by a certain formula first proposed for this purpose in the same Work (Art. 124, *et seq.*) The two Results, viz., the Discharge-measurements in the "flume" and over the "weirs" will be called for shortness the FLUME-DISCHARGE, and WEIR-DISCHARGE.

Comparing the Results in the 11 Experiments—

Flume-Discharge > Weir-Discharge in 9 cases; max. excess 4 per cent.

Flume-Discharge < Weir-Discharge in 2 cases; max. defect 1 per cent.

The excess in the 9 cases was always very small, viz.—

4 per cent., 2 cases; 3 per cent., 2 cases; 2 per cent., 2 cases; 1 per cent., 3 cases.

The Conclusions given are (Art. 184, *op. cit.*)—

"comparing all the results, however, we may say, *1st*, that, generally, there is a small excess in the results of the flume measurements, over that by the weirs; *2nd*, that this excess increases with the velocity in the flume; *3rd*, that the excess increases also with the difference between the length of the immersed part of the tubes, and the depth in the canal".

It appears to the present Author that it by no means follows that the Flume-Discharges were really larger than the True Discharge, as it may well happen that the Weir-Discharges (with which alone they can be compared) *are themselves a little too small.* And further, the number of Experiments was too small (only 11), and the *amount of excess too small* (see above) to warrant any broad generalizations. It may

* This term, FLUME, is now used by American hydraulicians in the sense of a short uniform channel prepared for Discharge-measurements.

well happen that the smaller excesses of 1 and 2 per cent. are mainly errors in the Weir-Discharge due to the disturbed state of the water (Art. 180, *op. cit.*) in the Weir-Basin.

[As the depth on the weirs in no case much exceeded one foot, (*ib.*,) an error of ·01 of a foot in determining the water-level would cause an error *exceeding* 1 *per cent.* in the Discharge given by the Weir-Formula.]

Again, the Flumes used were *extremely unfavorable* to the proper use of the Rods in the following points :—

1°. From their shortness, (only 140').

2°. From the very great differences in width of the Supply-channel (80'), of the Flumes themselves (20' and 10'), and of the Exit-channel (12'.)

3°. From the very unsymmetric position of the Supply-channel, Flumes, and Exit-channel : this was especially unfavorable with the 10' Flume, which (being on the left side of the lock-channel, whilst the exit-passage was on the right side) was directly opposite the obstruction by which the 20' channel was contracted to the 12' passage.

4°. From the very great changes of level from the bed of the Flume to the bed of the Locks (drop of 7'), and from the bed of the Locks to the sill of the three discharging Weirs (rise of 11'), all occurring within the short distance of 218' below the tail of the Flume.

This led to an extremely irregular distribution of the velocities across the channel, (as may be seen from Pl. XV of same Work), which was of course very unfavorable to the use of the Rods. The remarkable closeness of the agreement of the Flume- and Weir-Discharges under such circumstances is in fact greatly in favor of the sufficient practical agreement of both processes (without necessarily proving anything in favor of either).

No opinion is in fact given in the Lowell Experiments (1855) as to the comparative accuracy of the two Experiments; but it appears that the Author was subsequently convinced that Rod-velocities give too large Results. This appears from a passage in the Mississippi Report (p. 309) wherein a certain "co-efficient of correction" is proposed,—

$$\text{Co-efficient} = 1{\cdot}000 - 0{\cdot}116\left(\sqrt{1-\frac{l}{\mathrm{H}}} - 0{\cdot}1\right)$$

to be applied to Rod-velocities, upon (unpublished) information supplied by the Author of the Lowell Experiments. The Mississippi Writers are evidently (p. 309 *ibidem*) convinced of the propriety of some co-efficient* of reduction.

15b. *Rhine Experts.* (Verslag aan den Koning,† by Heemskerk, 1876).—It is herein stated (p. 192 of above)—

"In 1873, Discharges of the five branches of the upper Rhine were taken with Krayenhoff's staves : in the autumn Woltmann's meter was used. The results obtained in 1873 gave reason to conclude that the staves furnish data deviating widely from the indications obtained by the Woltmann meter which they exceed."

Again, it appears from (p. 193, *ib.*) that the Krayenhoff's staves were not considered trustworthy, but the details are not given on which either of these Conclusions were arrived at.

* The evidence of this co-efficient (which is one of *reduction*) is not known to the Author.

† This reference was kindly supplied (with translation) by Mr. R. Gordon.

CHAPTER XVI.

THEORY OF ROD-MOTION.

Preface.—This Chapter contains a mathematical investigation of the Theory of Rod-Motion. The reader who is not interested in the mathematical details should read only Art. 1, 10, 11, 12, which contain the practical Results.

1. Rod-Motion, *Fundamental Equation.*—The following is an investigation* of the question of the actual Rod-velocity (u), *i.e.*, of the real velocity of a long Rod *after attaining the state of relative equilibrium* with the current, after which of course the Total Forces of Acceleration (F) and Retardation (R) acting on it must be balanced, leading to the fundamental Equation, (compare Ch. XV, 3)—

$$F + R = 0, \quad\text{...(1)},$$

from which sole equation it is proposed to determine the Rod-velocity (u).

2. Notation, (Pl. LII).—The investigation being a somewhat complex one, it is well to take such axes of reference as will lead to simple expressions. For this reason the line (aA) of maximum velocity, and a vertical line through the vertex (A) of the curve, will be taken as co-ordinate axes.

[Positive ordinates are measured *downwards* from the axis aA, so that upward ordinates as ab in *Fig.* 3, 4, 5 are *negative*].

The following Notation will be used, (*see* Pl. LII)—

$Z =$ Distance (ab) of surface bB from axis aA, (negative in *Fig.* 3, 4, 5).
$Z' =$ Depth of foot l of Rod Ll below axis aA.
$h =$ Depth of Rod velocity-line cC below axis aA.
$k =$ „ „ „ „ below surface bB.
$h_0 =$ „ of Mean velocity-line below surface bB.
$l =$ „ of immersion of foot l of Rod „ „
$b =$ breadth of Rod.
$m =$ reciprocal of parameter ($1 \div p$) of velocity-parabola.
$v =$ velocity (pP) at any distance $ap = s$ from axis aA.
$v_0 =$ surface-velocity (bB) at distance $ab = +$ Z below axis in *Fig.* 1.
„ „ „ „ $= -$ Z above axis in *Fig.* 3, 4, 5.
$V =$ maximum velocity (aA).
$u =$ velocity (oC) of Rod Ll at distance $z = h$ below axis.
$F =$ Sum of Accelerating Forces on Rod, *i.e.*, on all parts wherein $v > u$.
$R =$ Sum of Retardating Forces on Rod, *i.e.*, on all parts wherein $v < u$.

3. General Expressions for F, R.—Now according to the usually accepted experimental Results on the effect of pressure and friction of a current *on a small*

* The investigation in this Chapter is now published (it is believed) for the first time. Most of the mathematical work has been verified by Lieut. J. H. C. Harrison, R.E.: the numerical details have all been verified by a good computer.

portion (of height dz) of a cylindric Rod (of diameter b) immersed in it, the combined* Resultant Pressure and Friction on it are proportional to the area directly exposed ($\pi b dz$) and to the square of the relative velocity $(v-u)$ or $(u-v)$ of the Rod itself (u) and of the current (v) at the part in question, *i.e.*,

Resultant Force on small segment $\pi b dz$ is $\begin{cases} \text{Acceleration} = \mu\,(v-u)^2\,b\,dz, \text{ when } v > u, \ldots\ldots (2a), \\ \text{Retardation} = -\mu\,(u-v)^2\,b\,dz, \text{ when } u > v, \ldots\ldots (2b), \end{cases}$

where μ is a constant depending on the nature of the surface of the Rod.

Hence the Resultant Force on a *finite* segment of length $(a-b)$ (*i.e.*, lying between the limits $z=a$, $z=b$) of such a Rod is—

Resultant Force on segment from $z=a$ to $z=b$ is $\begin{cases} \text{Acceleration, } F = \mu b \displaystyle\int_a^b (v-u)^2\,dz, \text{ when } v > u, \ (3a), \\ \text{Retardation, } R = \mu b \displaystyle\int_a^b (u-v)^2\,dz, \text{ when } u > v, \ (3b), \end{cases}$

or adopting the abbreviation $\int (v-u)^2\,dz = \phi\,(z), \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (4).$

Acceleration, $F = \mu b\,.\,[\phi\,(b) - \phi\,(a)]$, when $v > u$ throughout,..................(5a).

Retardation, $R = -\mu b\,.\,[\phi\,(b) - \phi\,(a)]$, when $u > v$ throughout,..................(5b).

It remains to apply these general expressions to the velocity-curve. To give the investigation a high degree of generality, it will only be assumed at this stage (from the results of experiment, *see* Ch. X, 8) that—

"The Average Velocity-Curve is everywhere convex down-stream",............(6), *i.e.*, of the general figure shown on Pl. LII.

Several principal cases occur according as the maximum velocity-line aA (Pl. LII) is above, in, or below the surface; and when below the surface there are three principal cases according as the Rod-velocity (u) is less than, equal to, or greater than, the Surface-velocity (v_0). These Cases are shown separately in *Fig.* 1—5 of Pl. LII; the Rod itself is shown by the thick line Ll, its head being L and foot l in each case, so that cC represents the Rod-velocity (u).

The following Table shows the values of the Resultant Forces of Acceleration (F) and Retardation (R) for each Case:—

CASE.	Fig. (Plate LII).	Position of Max. Velocity-Line.	Acceleration (F) and Retardation (R).			Reference.
			Segment.	Limits of z.	Values of F, R.	
i	1	Above surface, $u < v_0$	LC Cl	Z, h h, Z'	$F = \mu b\,[\phi\,(h) - \phi\,(Z)]$ $R = -\mu b\,[\phi\,(Z') - \phi\,(h)]$	(7a).
ii	2	In surface, $Z = 0$	LC Cl	$0, h$ h, Z'	$F = \mu b\,[\phi\,(h) - \phi\,(0)]$ $R = -\mu b\,[\phi\,(Z') - \phi\,(h)]$	(7b).
iii	3	Below surface, $u < v_0$	LC Cl	$-Z, h$ h, Z'	$F = \mu b\,[\phi\,(h) - \phi\,(-Z)]$ $R = -\mu b\,[\phi\,(Z') - \phi\,(h)]$	(7c).
iv	4	Below surface, $u = v_0$	LC Cl	$-Z = -h, h$ h, Z'	$F = \mu b\,[\phi\,(h) - \phi\,(-h)]$ $R = -\mu b\,[\phi\,(Z') - \phi\,(h)]$	(7d).
v	5	Below surface, $u > v_0$	C′C LC′ & Cl	$-h, h$ $-Z, -h$ & h, Z'	$F = \mu b\,[\phi\,(h) - \phi\,(-h)]$ $R = -\mu b\,[\phi\,(-h) - \phi(-Z)]$ $-\mu b\,[\phi\,(Z') - \phi\,(h)]$	(7e).

* *See* Rankine's "Manual of Applied Mechanics" (1872), Art. 652, for Results as to pressure, and Poncelet's "Introduction à la Mécanique Industrielle" (1844), Art 387, 388 for Results as to friction.

4. General Equations.—Substituting these values of F, R into the fundamental equation $F+R=0$, the following result as the equations of uniform motion in the five cases, (after division by μb, and re-arrangement).

Case i. $2\phi(h)-\phi(Z')-\phi(Z)=0$,..........(8*a*).
Case ii. $2\phi(h)-\phi(Z')-\phi(0)=0$,..........(8*b*).
Case iii. $2\phi(h)-\phi(Z')-\phi(-Z)=0$,..........(8*c*).
Case iv. $2\phi(h)-\phi(Z')-\phi(-h)=0$,..........(8*d*).
Case v. $2\phi(h)-\phi(Z')-2\phi(-h)+\phi(-Z)=0$,..........(8*e*).

There are some other Cases (varieties of Case v) arising only in the use of Short Rods for a special purpose, viz., when the maximum velocity-line is deep seated, and the Rod is too short to dip below the level *c*C. These cases are of little practical interest, as in practical use, the Rods would only be used nearly reaching the Bed.

The first two terms of the above five equations are identical. The first four equations may obviously be included under a single form, either (8*a*) or (8*c*) by simply varying Z; similarly Eq. (8*e*) merges into (8*d*) when $Z=h$. Thus there are two general types, viz., Eq. (8*a*) or (8*c*) and (8*e*) separated by the critical form (8*d*), into which they both merge when $Z=h$.

5. Velocity-Parabola.—The preceding Results are *perfectly general*, *i. e.*, they are independent of any hypothesis as to the figure of the Average Velocity-Curve (except that it is assumed convex). To produce any *definite solution*, *i. e.*, to enable the integration denoted by the symbol ϕ to be performed, some geometrical figure must be assigned for the Velocity-Curve, and in accordance with the Results of Ch. XI, 7 it will be assumed to be a common parabola.

Referred to the axes chosen, the equation of this parabola is—

$$V-v=ms^2, \qquad (9).$$

whence, $V-u=mh^2$, and $v-u=m(h^2-s^2)$, ...(9*a*).

Hence, $$\phi(s)=\int(v-u)^2\,ds=m\int^s(h^4-2h^2s^2+s^4)\,ds,$$
$$=m.(h^4s-\tfrac{2}{3}h^2s^3+\tfrac{1}{5}s^5), \qquad (10).$$

Hence also, $\phi(o)=0$, $\phi(h)=\frac{8}{15}mh^5$, ...(10*a*),
and, in general, $\phi(-s)=-\phi(s)$, ...(10*b*).

[This last property (10*b*) is in no way peculiar to the parabola, but is (as may be readily shown) common to any symmetric wholly convex figure].

6. Practical Limits.—As a preliminary step, and to confine the investigation within due bounds, it is necessary to assign some limits to the variation of position of the parabola-axis (defined by Z), upon which the position of the Rod-velocity line (defined by h) will be found to depend. It will be assumed as *known from Experiment* that (*see* Tab. 3, 4)—

"The elevation of the maximum velocity-line *above* the surface is not known to exceed the full depth (H) of water",.......... (11*a*).

"The depression of the maximum velocity-line below the surface is not known to exceed the half depth (H) of water", ... (11*b*),

or in symbols—

CASE i. *Axis above surface*, Z not > H, or not > $\frac{1}{2}$Z', ... (12*a*).
CASE v. *Axis below surface*, Z not > $\frac{1}{2}$H, or not > Z', ... (12*b*).

7. Parabolic Equations.—Introducing the values of $\phi(s)$, $\phi(h)$, &c., from Eq. (10—10*b*) into the general equations (8*a*—*e*) and attending to the limits of Z in each

case, these equations take the following special forms (after reduction and arrangement) due to the parabolic figure of curve. A sixth Case has been added corresponding to the lowest (mid-depth) position of the axis to complete the scheme.

CASE.	Limits of Z.	Parabolic Equations.	Solution [Value of $h \div Z'$]. see Art. 8, 9.	Result.
i	$\frac{1}{2}Z'$ to 0	$16h^5 - 15(Z'+Z)h^4 + 10(Z'^3+Z^3)h^3 - 3(Z'^5+Z^5) = 0$	depends on $Z \div Z'$	(18*a*).
ii	$Z = 0$	$16h^5 - 15Z'h^4 + 10Z'^3h^3 - 3Z'^5 = 0$	·6106	(18*b*).
iii	0 to h	$16h^5 - 15(Z'-Z)h^4 + 10(Z'^3-Z^3)h^3 - 3(Z'^5-Z^5) = 0$	depends on $Z \div Z'$	(18*c*).
iv	$Z = h$	$24h^5 - 15Z'h^4 + 10Z'^3h^3 - 3Z'^5 = 0$	·5615	(18*d*).
v	h to Z'	$32h^5 - 15(Z'+Z)h^4 + 10(Z'^3+Z^3)h^3 - 3(Z'^5+Z^5) = 0$	depends on $Z \div Z'$	(18*e*).
vi	$Z = Z'$	$16h^5 - 15Z'h^4 + 10Z'^3h^3 - 3Z'^5 = 0$	·6106	(18*f*).

The equations are seen to be all homogeneous, and therefore suffice (except for algebraic difficulties) for determining either of the ratios $h \div Z$, $h \div Z'$ in terms of the ratio $Z \div Z'$.

This shows that—

"The relative position of the Rod-velocity-line (defined by the ratios $h \div Z$, $h \div Z'$) depends only on the relative position of the max. velocity-line (defined by $Z \div Z'$)", ...(14).

This is of course analogous to the similar property of the Mean Velocity Line, (Ch. XIV, 9d, (37*a*)). As before remarked, the Equations fall under *two* types, viz., (18*c*), (18*e*), all the rest being obtainable from these two by simple variation of Z; these distinct types being separated by the critical form (18*d*) into which both merge when $Z = h$. It is worth remarking also that the final form (when $Z = Z'$) is the same as the form given by $Z = 0$.

8. Solution.—As a preliminary to practical solution, it is convenient to divide the equations through by Z'^5, upon which they appear as equations giving the ratio $h \div Z'$ in terms of the ratio $Z \div Z'$. The equations being of fifth degree in both ratios, it is *impossible to solve them in a general manner*, so as to exhibit the value of the ratio $h \div Z'$ in terms of the ratio $Z \div Z'$. But definite *numerical* solutions may be obtained for any given *numerical* values* of the ratio $Z \div Z'$, and this will serve equally well for all practical purposes.

It may be shown* by the Theory of Equations that—within the assigned limits of Z—

"All the equations have one, and only one positive root",.....................(15*a*),

"That root (value of $h \div Z'$) lies between 0 and + 1",...........................(15*b*),

the physical meaning of which is that below the maximum velocity-line there is a single stream-line in which the velocity is equal to the Rod-velocity, a Result which is otherwise obvious from physical considerations.

The solution of the three Cases ii, iv, vi in which definite numerical values are assigned to Z may be shown* to be $h \div Z' =$ ·6106, ·5615, ·6106 respectively as given in Table above.

* *See* any Work on the Theory of Equations. The numerical detail involved in the process is so heavy that it is not given here; suffice to say, that it has been carefully checked by a computer.

9. Practical Solution.—It remains to transform the solution just found so as to exhibit the depth (k) of the Rod-velocity *below the surface* (instead of the depth (h) below the axis of the curve as given by above), as more convenient for practical purposes, and to compare this with the depth (h_0) of the Mean Velocity below the surface. For this purpose it will obviously suffice to compute the ratio $k \div l$, and compare it with the ratio $h_0 \div \mathrm{H}$.

Observing that in forming the expressions (7*a*—*e*) the algebraic signs $\pm$ have already been attached to the quantities h, Z *when measured upwards*, it is clear that the quantities h, Z, in the resulting equations (8*a*—*e*), (13*a*—*f*) are to be *reckoned positive*, so that—

$$k = \mathrm{Z} + h, \quad l = \mathrm{Z} + \mathrm{Z}', \text{......} (16a),$$

whence,
$$\frac{\mathrm{Z}}{l} = \frac{\mathrm{Z}}{\mathrm{Z} + \mathrm{Z}'}, \quad \text{and} \quad \frac{k}{l} = \frac{\mathrm{Z} + h}{\mathrm{Z} + \mathrm{Z}'}, \text{......} (16b).$$

Hence the values of $\mathrm{Z} \div l$, $k \div l$ for the three definite Cases ii, iv, vi of Art. 7, are as shown below: the values of $h_0 \div l$ have been found by making $\mathrm{H} = l$ in the Results of Ch. XIV, 9d.

Case.	$\mathrm{Z} \div \mathrm{Z}'$.	$h \div \mathrm{Z}'$.	$\mathrm{Z} \div l$.	$k \div l$.	$h_0 \div l$.	Differences ($k \div l - h_0 \div l$).	Result.
ii,	0	·6106	0	·6106	·577	·034	(17*b*).
iv,	·5615	·5615	·3596	·7192	·681	·038	(17*d*).
vi,	1·0	·6106	·5	·8053	·789	·016	(17*f*).

The two Columns $k \div l$, $h_0 \div l$ of the Table show the relative depths of the Rod-velocity (viz., $k \div l$) and Mean velocity (viz., $h_0 \div l$) corresponding to the three relative depths of the maximum velocity-line (viz., $\mathrm{Z} \div l$) of Cases ii, iv, vi, (viz., $\mathrm{Z} \div l = 0$, ·3596, ·5). It will be seen that $k \div l > h_0 \div l$ in each of these three cases. And from the *continuity of the expressions* in Eq. (18*a*—*f*), (each passing into the next by variation of Z as shown in Art. 7,) it follows that this property is generally true, viz.—

"The ratio $k \div l > h_0 \div l$ (within the limits of Z assigned) by a few hundredths, not exceeding ·04",......(18*a*).

10. Practical Results.—The last Result may be thus expressed—

"The Rod velocity-line is—within the limits of practice (Art. 6)—always *somewhat more deeply seated* than the line of Mean Velocity past the immersed portion of the Rod",......(18*b*).

This shows at once, attending to the figure of the Vertical Velocity-Curve, that—

"The Rod-velocity is—within the limits of practice (Art. 6)—always *somewhat less* than the Mean Velocity past the immersed portion ($\mathrm{L}l$) of the Rod",....(18*c*).

From the above the following important practical inference may be at once drawn :—

"The depth of immersion (l) of a Rod should be—for mere accuracy of measurement of Mean Velocity past a vertical—decidedly less than the full depth (H) on that vertical",..(19).

Inasmuch as the practical use of a Rod *necessarily* entails this very same state of things (viz., depth of immersion < full depth), the above cannot fail to be regarded as a most important practical Result, inasmuch as it removes what has hitherto been always supposed to be a serious objection to the use of Rods on the score* of accuracy.

11. Proper Rod-Length.—To give the above Result its full practical value, it would be desirable to show—if possible—what length of Rod (l) should be used in a given depth (H) of water. Unfortunately the nature of the case does not admit of this being done *definitively*, inasmuch as the ratios $k \div l$, $h_0 \div l$, $k \div h_0$ are not constant, but depend on the relative depth ($Z \div l$) of the maximum velocity, a ratio which is both variable and *unknown à priori.*

But the desired Result may be obtained *sufficiently nearly for most practical purposes as follows* :—

For Eq. (35) of Ch. XIV gives h_0 in terms of Z and H ; also the accurate use of the Rods requires that the length of Rod (l) should be such that the Rod-velocity and Mean Velocity-lines should coincide, or $k = h_0$. Substituting this value ($k = h_0$) into the Result preceding Eq. (35) of Ch. XIV, there results—

$$k^2 - 2Zk = \tfrac{1}{3}H^2 - ZH, \qquad (20).$$

This may be written, $$\left\{\left(\frac{k}{l}\right)^2 - 2\frac{Z}{l}\cdot\frac{k}{l}\right\}\cdot\frac{l^2}{H^2} + \frac{Z}{l}\cdot\frac{l}{H} = \frac{1}{3}, \qquad (20a).$$

This is the final equation from which the value of $l \div H$ may be found when the values of $k \div l$ are known for given values of $z \div l$. The values of $k \div l$ resulting from solution of this equation for the 3 values of $z \div l$ found in Art. 9, are shown below—

Case.	$Z \div l$.	$k \div l$.	$l \div H$.	$Z \div H$.	$k \div H$ or $h_0 \div H$.	Result.
ii,	0	·6106	·945	0	·577	(21b).
iv,	·3596	·7192	·927	·333	·667	(21d).
vi,	·5	·8053	·950	·475	·765	(21f).

The relative depths of the maximum velocity-line (viz., $Z \div H$), and of the coincident Rod-velocity and Mean velocity-lines (viz., $k \div H$ and $h_0 \div H$) are also shown

* it having been supposed hitherto (*see* Ch. XV, 14b) that a Rod only slightly shorter than the full depth necessarily moved faster than the Mean Velocity past the vertical.

in the two last columns: the former ($Z \div H$) was found by multiplying the known ratios $\frac{Z}{l} \times \frac{l}{H}$, and the two latter ($k \div H$, $k_0 \div H$) by solution of Eq. (20). From the argument of "continuity" of the algebraic expressions already used in Art. 9, it equally follows here that the ratios $l \div H$, $Z \div H$, $k \div H$ will vary continuously.

Thus it appears that—

"The proper length of Rod should vary from ·945 to ·927 of the full depth as the maximum velocity-line sinks from the surface to ¼-depth, and from ·927 to ·950 of the full depth as the maximum velocity-line sinks from ¼-depth to about mid-depth",..(22*a*).

The variation from ·950 to ·927 is so slight, that for all practical purposes it will probably be a sufficient approximation to use an average value say ·94, in all cases, thus—

"The proper length of Rod is approximately ·94 of the full depth of water",(22*b*).

And seeing that the depth of the maximum velocity-line upon which this ratio depends is unknown *à priori*, the *approximate constancy* thereof (within the limits of practice, Art. 6), is an important practical Result, as removing any uncertainty as to the proper length of Rod.

[It may be objected that no practical use has been made of the above Results (22*a*, *b*) in these Experiments, (the Rods used having been made up in Sets of fixed lengths, Ch. XV, 6). Unfortunately the possibility of bringing this investigation (which is believed to be quite new) to a successful issue, only occurred to the Author after the closure of the Field-work. It is, moreover, doubtful whether practical convenience would admit of frequently adjusting the Rod-lengths to a variable depth of water, as required by Results (22*a*, *b*). Rods of fixed lengths varying by *small increments* would be more convenient for practical use, and would probably meet the requirements of Results (22*a*, *b*) sufficiently for practical purposes if the increments were small enough (say 3")].

12. *Results true on the average.*—The whole of the numerical Results of this Chapter depend upon the approximately parabolic figure (now generally admitted) of the Average Vertical Velocity-Curve, and are therefore so far only *shown true as Average Results*, and cannot be expected to obtain in single velocity-measurements: but in repeated velocity-measurements they may be accepted with confidence.

END OF PART II.

PART III.

CHAPTER XVII.

TRANSVERSE VELOCITY-CURVES.

Preface.—This Chapter contains full details of the Experiments (Art. 1—7) for tracing the figure of the Transverse Velocity-Curves with detailed discussion of their chief properties (Art. 8—15). The most important Articles are Art. 1—3, 5, 6, 6b, 6c, 8—9b, 12, 13, 14b, 15.

1. Transverse Velocity-Curves.—The meaning of this term has been defined (Ch. I, 9) as—

"Curves whose ordinates are the 'forward velocities' at all points of a Transverse Base Line in a cross-section".

The importance of these Curves to both Theory and Practice has already been explained in Ch. I, 10. Much and continuous Experiment was accordingly devoted to this research. The Curves studied were—

1°, *Surface Velocity-Curves*, at the Soláni Aqueduct and Embankt. (Minor) Sites,
2°, *Mid-depth Velocity-Curves*, at the Soláni Right Aqueduct Site,
3°, *Bed Velocity-Curves*, at the Soláni Right Aqueduct Site,
4°, *Mean Velocity-Curves*, at all the Sites, (except the Soláni Embankment Minor.)

The three former are probably the most important for THEORY; but the last is so much the most important for PRACTICE, from its giving the data for computing the Cubic Discharge, (the most important of all the Hydraulic Results,) that by far the greater portion of the *systematic* Experiment was devoted to it.

The Experiments were made under varying conditions of water-level at each Site as shown in Tables A, B on pp. 248, 249.

The following is an Abstract of the Tables in question:—

Surface-Velocity work,	10 Series	comprising	109 Sets at	4 Sites,
Mid-depth-Velocity work,	2 Series	„	17 Sets at	1 Site,
Bed-Velocity work,	2 Series	„	7 Sets at	1 Site,
Mean Velocity work,	100 Series	„	581 Sets at	11 Sites,

showing that by far the greater part of the time was devoted to the last (the most important work). But not only was there far more in quantity of the latter work, but it was done *under much more varied conditions* than any of the others (*see* Tab. 32): the work for instance in the Right Soláni Aqueduct with the Left Aqueduct closed, and also the work at

very low water were solely of this kind; these conditions being both of rare occurrence, and therefore proportionately valuable when they did occur. This is believed to be a Collection both larger and under more varied conditions than has ever before been published.

2. Instruments.—The velocity-measurements were made with the following Instruments :—

Surface Velocities, 8″ × ¼″ pine Discs, (Ch. IV, 13.)
Mid-depth Velocities, 1¼″ DOUBLE-FLOATS, (Ch. IX, 12b.)
Bed Velocities, 1¼″ DOUBLE-FLOATS, (Ch. IX, 12b.)
Mean Velocities, 1″ wood RODS in 1874-76, (Ch. XV, 7a.)
" " 2½″ wood RODS in 1875, (Ch. XV, 7b.)
" " 1″ tin TUBE-RODS in 1876-79, (Ch. XV, 7c.)
" " 1″ wood RODS (under 1′ length) in 1876-79, (Ch. XV, 7a).

3. Mode of research.—The mode of research employed (explained in a general way in Ch. I, 11) was to measure the "velocity" of any one kind, viz., surface-, mid-depth-, bed-, or mean velocity (past a vertical) at many points of the same transversal, *e. g.*, at the centre and at many points (from 5 to 10) on either side of the centre, thus giving the values of many ordinates (from 11 to 21) of each Transverse Velocity-Curve.

Table A *referred to in Art.* 1.

SURFACE VELOCITY-WORK.												MID-DEPTH VELOCITY-WORK.				BED VELOCITY-WORK.			
Soláni Left Aqueduct.				Soláni Right Aqueduct.				Soláni Embankt. [Minor Sites.]				Soláni Right Aqueduct.				Soláni Right Aqueduct.			
Table.	Serial No.	Number of Sets.	Average Gauge.	Table.	Serial No.	Number of Sets.	Average Gauge.	Table.	Serial No.	Number of Sets.	Average Gauge.	Table.	Serial No.	Number of Sets.	Average Gauge.	Table.	Serial No.	Number of Sets.	Average Gauge.
XXIX	51	15	8·97	XXX	53	16	9·90	XXXIII	60	10	9·59	XXXII	61	16	10·00	XXXII	65	3	10·00
	52	14	8·83		54	4	9·10						62	1	9·05		66	4	8·77
					55	1	8·78												
				XXXI	56	16	8·71												
					57	16	8·55												
					58	14	8·07												
					59	3	7·57												
Total, 2		29	..	Total, 7		70	..	Total, 1		10	..	Total, 2		17	..	Total, 2		7	..

[Ser. 51, 52, and part of 54, 56, 60, were published in the 1874-5 Report, (where they are numbered 5, 6, 8, 7, 3, respectively): they are reprinted now—with some modifications (and corrections)—for completeness' sake].

Table B *referred to in Art.* 1.

ABSTRACT OF MEAN VELOCITY WORK.

Sites.	Table.	Serial No.	Number of Sets.	Average Gauge.
SOLANI LEFT AQUEDUCT.	XXXIV	101	3	9·90
	„	102	12	9·63
	„	103	4	9·42
	XXXV	104	12	9·05
	„	105	2	8·61
	„	106	6	8·02
	„	107	6	7·50
Total, ...		7	45	...
SOLANI RIGHT AQUEDUCT. [Left Aqueduct open].	XXXVI	108	19	9·96
	„	109	18	9·61
	XXXVII	110	20	9·33
	„	111	16	8·97
	XXXVIII	112	18	8·58
	„	113	1	8·16
	„	114	20	7·98
	XXXIX	115	1	7·80
	„	116	7	7·49
	„	117	2	7·09
	„	118	16	6·67
	„	119	7	6·15
	XL	120	9	5·78
	„	121	2	5·61
	„	122	13	4·48
	„	123	1	3·65
	„	124	1	3·49
	„	125	1	2·02
	„	126	1	1·92
	„	127	1	·70
Total, ..		20	174	...
[L. Aqueduct closed].	XLI	131	2	4·60
	„	132	2	3·96
	„	133	1	3·60
	„	134	1	3·58
	„	135	1	3·18
	„	136	1	3·12
	„	137	1	3·13
	„	138	1	2·88
	„	139	2	2·66
Total, ...		9	12	...

Sites.	Table.	Serial No.	Number of Sets.	Average Gauge.
SOLANI EMBANKMENT MAIN SITE. HIGH WATER.	XLII	151	5	9·94
	„	152	17	9·91
	„	153	6	9·52
	XLIII	154	5	9·08
	„	155	6	8·74
	„	156	4	8·43
	„	157	7	8·12
	„	158	2	7·90
	XLIV	159	9	7·59
	„	160	6	7·06
	„	161	11	6·82
	XLV	162	5	6·38
	„	163	6	5·58
	„	164	1	5·24
	„	165	6	4·96
	„	166	3	4·41
LOW WATER.	XLVI	167	10	4·01
	„	168	1	3·98
	„	169	10	3·74
	„	170	2	3·64
	XLVII	171	3	3·62
	„	172	2	3·58
	„	173	5	3·47
	„	174	1	3·04
	„	175	5	2·90
	XLVIII	176	2	2·83
	„	177	4	2·42
	„	178	2	2·43
	„	179	4	1·92
	„	180	2	1·67
	„	181	1	·35
Total, ...		31	153	...
15TH MILE.	XLIX	191	1	15·31
	„	192	6	14·32
	„	193	3	13·99
	„	194	2	13·60
	„	195	1	12·53
	„	196	3	15·16
	„	197	1	14·69
Total, ...		7	17	...

Sites.	Table.	Serial No.	Number of Sets.	Average Gauge.
BELRA.	L	201	5	7·44
	„	202	7	7·03
	„	203	9	6·80
	LI	204	14	6·49
	„	205	6	6·30
	„	206	12	5·84
Total, ...		6	53	...
JAOLI.	LII	211	9	7·21
	„	212	6	6·71
	„	213	7	6·45
	„	214	8	6·21
	LIII	215	10	5·97
	„	216	9	5·64
	„	217	6	5·36
Total, ..		7	55	...
KAMHERA.	LIV	221	4	6·46
	„	222	15	6·07
	„	223	11	5·79
	LV	224	12	5·53
	„	225	14	5·34
Total, ...		5	56	...
DISTRIBUTARIES.	LVI	231	2	4·35
	„	232	2	3·10
	„	233	4	3·97
	„	234	1	3·60
	„	235	1	3·75
	„	236	2	2·83
	„	237	2	3·79
	„	238	2	3·33
Total, ...		8	16	...

[Ser. 102, 104 were published in the 1874-75 Report, (where they are numbered 16R, 15R respectively): they are reprinted now—with some modifications (and corrections)—for the sake of completeness].

4. *Subsurface Transversals.*—The Instrument (the Double-Float) employed has of course some effect on the Results obtained. It has been explained (Ch. X, 4) that the convenient use of the Double-Float in large numbers does not admit of frequent change of the length of its Connector. In the Mid-depth and Bed Velocity work the length (l) of all the Connectors was accordingly adjusted roughly to the half depth ($\frac{1}{2}$H), and full depth (H) for the work of each Series once for all, viz., *see* Col. 2, Tab. XXXII,—

Mid-depth work, $l = 5'$ in depths ranging from 10'·10 to 9'·86, (Ser. 61.)
$l = 4\frac{1}{2}'$, in a depth of 9'·05, (Ser. 62.)
Bed work, $l = 10'$, in a depth of 10'·00, (Ser. 65.)
$l = 8'$, in depths ranging from 8'·95 to 8'·68, (Ser. 66.)

Moreover, the "Lift" of the Sub-Float causes the velocity-measurements to be made (*see* Ch. IX, 8, iv—vi) at a higher level than that indicated by the full length of the Connector. Thus the so-called Mid-depth and Bed Velocity work was really effected upon a Transversal *probably somewhat above* the Mid-depth and Bed respectively.

For shortness' sake the terms MID-DEPTH- and BED-Transversal, -Velocity, and Velocity-Curve will, however, be used in what follows, the above reservation being understood.

5. Ordinate-system.—It is obvious that, when the figure of a curve is to be determined from a limited number of ordinates, it is desirable that—

"The ordinates should be closest together where their change is most rapid",...(1).

Moreover, as the figure of the bed may be expected to determine the figure of the velocity-curve, and as every marked change in the figure of the bed may therefore be expected to affect the figure of the velocity-curve, the ordinate-spacing should be such as to exhibit the relation if any.

Artificial channels also are generally of following character:—

1°, of roughly symmetric cross-section.
2°, with a roughly level bed of great width (compared with depth of channel).
3°, with (roughly speaking) only two abrupt changes of figure in the contour of the bed, viz., at the foot of each bank.
4°, with steep banks.

These features may be expected to (and indeed do) produce corresponding features in the Transverse Velocity-Curves, viz.—

1°, a certain amount of symmetry.
2°, flatness (of curve) over the level bed.
3°, a marked change about the foot of each bank.
4°, rapid change over the steep banks.

To bring out these features, it seems desirable that the ordinate-spacing should be—

1°, symmetrical about the central line of the bed.
2°, wide spaced over the level bed.

3°, closer spaced with approach to the banks, and with one ordinate at the foot of each bank.

4°, closest spaced nearest the edge.

5a. *Calculation requirements.*—One of the objects of the research being the calculation of the Cross-Section Areas, and also of the superficial Discharges past the several Transversals, and of the Cubic Discharge, (the two latter measured by the Velocity-Curve Area and Velocity-Surface Volume respectively,) it is convenient to divide the Transversals into several *primary* segments, within which severally the ordinates shall be spaced *at equal distances*, (because the approximation-formulæ to be used require equidistant ordinates). These primary divisions of the Transversal will for shortness be called SPACES, and the equal sub-divisions of the Spaces will for shortness be called SUB-SPACES.

Of the available formulæ, moreover—viz., (*see* Ch. XIII, 2a) the Trapezoidal, Parabolic (Simson's), Cubic and Sextic (Weddle's)—the Trapezoidal being the least accurate, it is desirable to adopt a mode of subdivision suitable for the three latter, *i. e.*—

"The Spaces should be sub-divided into a number (n) of èqual Sub-spaces, which is a multiple of 2, 3, or 6", ... (2).

Practical convenience of calculation also requires that the Spaces and Sub-spaces should be multiples or convenient fractions of 10 feet in wide channels, or of 1 foot in narrow channels.

5b. *Float-Courses.*—It will be understood that the Float-Courses themselves (marked in the Field by the Lines of Pendants) are the division-lines between the various Sub-spaces, so that the velocity-measurements therein become the velocity-ordinates of the Curves.

5c. *Gaussian Spacing.*—It is to be remarked that the equidistant spacing is not the sub-division best suited for the purpose of Area- and Discharge-computations. Were these the only results sought, the spacing proposed by the mathematician Gauss should be adopted. This gives about the same degree of approximation* with use of only n ordinates as is obtained by the use of $2n$ equidistant ordinates. But the spacing is inconvenient if the figure of the Curve is one of the objects of investigation, as the abscissæ (y) are all inconvenient fractions of the breadth (b) of a space. It was accordingly not adopted in these Experiments.

It may be worth while quoting† the Gaussian Formulæ for use in any case where great accuracy of computation is desired: the middle line is taken as axis ($y = 0$).

Let b = breadth of whole space whose Area is required.

$v_{\pm y}$ = velocity at distance ($\pm\, y$) from middle line of the space.

n = number of velocity-ordinates, (excluding those at the edges.)

$n+1$ = number of sub-spaces in the width b.

* Todhunter's Treatise on Laplace's, Lamé's and Bessel's Functions, (1875), Art. 122, 126.

† Same Work, Art. 136.

n	Area of Curve from $y = -\frac{1}{2}b$ to $y = +\frac{1}{2}b$.
1	$v_0 \, . \, b$.
2	$\frac{1}{2}(v_{-.289b} + v_{+.289b}) \, . \, b$.
3	$\left\{\frac{5}{18}(v_{-.387b} + v_{+.387b}) + \frac{4}{9}v_0\right\} . \, b$.
4	$\left\{\cdot174\,(v_{-.431b} + v_{+.431b}) + \cdot326\,(v_{-.170b} + v_{+.170b})\right\} . \, b$.
5	$\left\{\cdot118\,(v_{-.453b} + v_{+.453b}) + \cdot239\,(v_{-.269b} + v_{+.269b}) + \cdot284\,v_0\right\} . \, b$.
6	$\left\{\cdot086\,(v_{-.466b} + v_{+.466b}) + \cdot180\,(v_{-.331b} + v_{+.331b}) + \cdot234\,(v_{-.119b} + v_{+.119b})\right\} . \, b$.
7	$\left\{\cdot065\,(v_{-.475b} + v_{+.475b}) + \cdot140\,(v_{-.371b} + v_{+.371b}) + \cdot191\,(v_{-.203b} + v_{+.203b}) + \cdot209\,v_0\right\} . \, b$.

6. Present Work, Spacing, Abstr. Tab. 11, & Pl. XXVI—XLI.—In the present Experiments—which were all in Canals (of figure described in Art. 5), with tolerably symmetric cross-section, and with a tolerably level bed of considerable width (compared with the depth of channel) and with steep banks—the arrangement of the Float-courses was always symmetric about a central Float-course over the centre line of the bed, and at Sites with sloping or stepped banks there was always one Float-Course over or near the foot of each bank.

The details differed somewhat for the wide Channels and Distributaries, being much simpler for the latter.

6a. DISTRIBUTARIES, (Tab. 11, & Pl. XLI).—A few Experiments only (16 in all) were made in these, solely with the view of obtaining a Discharge-measurement. A comparatively simple division was therefore thought sufficient (*see* Tab. 11). The Central Space occupied the greater part of the width, and was sub-divided into 10 or 12 equal Sub-spaces (so as to admit of use of Simson's or Weddle's Rules) by the Float-courses. The Side-spaces occupied the narrow spaces left near and over the Side-slopes: no attempt was made to sub-divide these.

6b. WIDE CHANNELS, (Tab. 11, & Pl. XXVI—XL).—The primary division of the Transversal was into either 5 or 7 unequal primary Spaces, which will be called for shortness the CENTRE SPACE, and the INTER- and SIDE-SPACES (to right and left respectively); the two former were each sub-divided into a certain number (n) of equal SUB-SPACES, n being always a multiple of 2, 3, or 6.

The Centre-Space, which was much the widest, was sub-divided into 6 or 8 equal Sub-spaces, (so as to admit of use of Weddle's or Simson's Rules.)

The Inter-Spaces were sub-divided into 2, 3, or 4 equal Sub-spaces, (so as to admit of the use of Simson's or the Cubic Rules.)

It will be seen that the Float-courses marking the division-lines within these (the Centre- and two Inter-) Spaces were always *at fixed points*, *i.e.*, at constant distances (say $\pm y$) from the centre, in each Site.

6c. SIDE-SPACES, (Wide Channels).—With one or two exceptions (noted below, in which no sub-division was attempted) these were sub-divided variously according to the different circumstances of each Site, (the details are seen at a glance in Tab. 11,) but always with a view to sub-division into 2 or 3 equal or nearly equal Sub-spaces, (so as to admit of the use of Simson's or the Cubic Rules,) and in some cases the outer Sub-space was again bisected, (as at q, Pl. XXV.)

6d. *Stepped and Sloping Banks.*—In Sites with stepped or sloping banks the surface-width, and therefore also the width of the Side-spaces, varied with the varying water-level. In these cases the Float-courses at the middle (m), and quarter (q) of the Side-spaces (Pl. XXV) varied also in position; for the sake of distinctness these Float-courses, and the points, pendants, depths and velocities corresponding will be distinguished by the letters m and q respectively through all the Tables, Plates and Text in which they appear.

[*Ex.*—This case occurs at the Solání Embankment Main Site, and in most of the earthen channels, *see* Tab. 11, & Pl. XXV].

6e. *Vertical Banks.*—In Sites with vertical banks (or with banks which were vertical or nearly so throughout the range of water-level occurring) the Float-courses in the Side-spaces were conveniently placed at fixed points.

[*Ex.*—This case occurs at the 15th Mile Old Site (Pl. XXXVII), Solání Embankment Minor Sites (Pl. XXVIII), and also Main Site at low water (Pl. XXXVI), and in the Solání Twin Aqueducts (Pl. XXVI, XXVII, & XXIX—XXXIII); or *see* Tab. 11 for all Sites].

6f. SPECIAL CASES.—There are two in number:—

1°. *Solání Twin Aqueducts.* The 8′ corbelling of the footways on the two outer banks (Pl. II, 4) prevented any velocity-measurements whatever being made within 8′ of the outer walls: they were made as close to the edge of the corbelling as possible, and are figured in the Tables and Plates *as if made at 39½′ from the centre,* (*i.e.*, at the very edge of the corbelling, or 8′ from the vertical wall.)

2°. *Solání Embankment Sites.* The Float-course spacing in the case of the Minor Sites (surface velocity work only), and also in the case of the Main Site at low water (no steps immersed), is sufficiently explained in Art. 6e, and Tab. 11.

But in the more important case of Mean Velocity work at the Main Site at high water (water surface above lowest step), it was thought desirable to trace the effect of the great variation in depth from step to step, and also of the abrupt break in the wet contour of the bed caused by the 4′ drop-wall (Pl. II, 2) with some care. With this view whenever the water covered any steps, the Float-courses in the Side-spaces were arranged as follows, (*see* Pl. XXV)—

One at 74½′ on either side of the centre, *i.e.*, 6″ within the drop-wall.

One at 75½′ on either side of the centre, *i.e.*, over middle of lowest step.

One over centre of top immersed step, (if more than one step was immersed.)

One half way between the two last, (if more than two steps were immersed): this is the m-point.

One half way between the two last, (if more than three steps were immersed): this is the q-point.

[The Rods used were of course of lengths suited to the depths over the several steps: those over the top immersed step were the small series under 1′ length (Art. 2); in very shallow water surface-floats had to be used].

Thus there were 1, 2, 3, or 4 distinct velocity-ordinates obtained over each Flight of Steps according as only 1, 2, 3, or more than 3 Steps were immersed of either Flight. These arrangements will be clear from Pl. XXV.

[The Treads of the Steps on the right bank were somewhat lower (about ·15′) than the corresponding Treads on the left bank, so that it often happened that more steps were immersed on the right than on the left bank, *see* Ser. 151, 154, 156, 159, 160, Pl. XXXIV, XXXV].

7. Field-work.—The mode and order of the Field-work has already been explained in Ch. VI, 12—12b, *q.v.*

The velocity-measurements were done as nearly in the order there explained, viz.,

> "3 at point nearest left bank, 3 at next point, 3 at next point, and so on in succession, ending with the point nearest right bank,"—

as was compatible with practical convenience. Slight modifications of two kinds were admitted—

1°. It frequently happened—in consequence of the Unsteady Motion of the water—that FLOATS arrived at the Upper Rope close to Pendants out of the proper order above given. This happened very often near the banks where the Float-Courses were closest together. The actual practice was to record the passage of any FLOAT that passed "in fair course" in any Float-Course even if "out of turn"; (provided of course that the Float in use was a suitable one for the Float-course in question).

2°. In using RODS in channels with uneven bed, the length of Rod required for each Float-Course depends on the Average Depth therein. When the whole available stock of any particular length of Rod had been cast from the Upper Boat, the custom was (if at a distance from the banks) to pass on to the next nearest Float-Course in turn, for which Rods of suitable length were available in the Upper Boat: so that the Boats were sent to bank for exchange of Rods as seldom as possible when at work at a distance from bank.

Much time was saved by both these practices. To carry out the first well, a good deal of care and skill is necessary on the part of both Caller and Timekeeper; as it would often happen that several Floats would arrive at the Upper Rope nearly together, but *in different Float-Courses;* so that they had to be recorded in different lines of the Field-book, (Ch. IV, 33, & Tab. **33**.)

8. Tabulation, Series, (Tab. XXIX—LVII, and **13—18**).—Tables XXIX—LVII contain the whole of the DETAILS connected with the Transverse Velocity-Curves.

The mode and order of tabulation of the SETS, and the combinations used in forming them into SERIES, have been explained in Ch. VI, 12c, 18a, *q.v.* These Articles include also the explanation of most of the Columns of these Tables containing the Observation-Data, (*see* also pp. 57, 67 of the Tables.) The explanation of the Fall of Water-Surface Column (No. **3**) will be found in Chap. VII, 9a. The explanation of the Result-Columns will be given in due course.

To save unnecessary looking over details, Abstracts have been prepared (Tab. **13—18**) showing the MEANS and RANGES of the Results for each Series.

9. Average Transverse Velocity-Curves, (Pl. XXVI—XLI).—The Plates show the Mean Results from the Detailed Tables (XXIX—LVII). Each Plate contains one or more AVERAGE CROSS-SECTIONS of the Site at the foot, and several Diagrams of AVERAGE VELOCITY-CURVES above.

9a. *Cross-Sections.*—Each Diagram shows one or more AVERAGE CROSS-SECTIONS of the bed and banks (as explained in Ch. V, 14—14b) at the Site, with the level of the

two Ropes shown by the (upper) dotted line, on which the positions of the Float-courses are shown (by short verticals), and their distances from the centre also figured. Two water-levels—the highest and lowest of the Plate—are also shown on each.

The position and depth of immersion of the Double-Floats, or Rods used, are also indicated by the vertical lines drawn down from the upper water-surface: in fact these verticals may be looked on as *pictorial representations of the Instruments* used.

The Cross-Sections in the wide channels are all on scale of 25 feet to an inch, and in the Distributaries, (Pl. XLI,) on the scale of 10 feet to an inch.

9b. *Velocity-Curves.*—Each Diagram shows the Average Velocity-measurements at the several points indicated on the Cross-Section in clear black lines plotted as ordinates from the Base-Transversal, close to which the Average Velocity-measurements themselves (taken from line v of the Detailed Tables) are figured.

The tips of the velocity-ordinates are joined by clear straight lines: the irregular line so formed is the AVERAGE TRANSVERSE VELOCITY-CURVE given by the data; the junction lines being all *straight*, the irregularities in the Observation-Curve are clearly exhibited, without the introduction of any bias of the draughtsman's hand (which inevitably results when free-hand curves are drawn).

Mean Velocity Line.—The Mean Velocity peculiar to each sort of Curve, viz.—

1°, Mean Surface Velocity, (U_0); 2°, Mean Mid-depth Velocity, ($U_{\frac{1}{2}H}$);

3°, Mean Bed Velocity (U_H); 4°, Mean (Sectional) Velocity, (V),

has also been plotted from the Base-Transversal upon the central velocity-ordinate; its tip is indicated by an arrow, and also by a clear line drawn right across the Curve; this line has an important physical bearing, viz.—

Curves 1°, 2°, 3°. The rectangle included between the line in question, the two dotted lines at the ends of the Base-Transversal, and the latter line itself, is equal to the Area of the Transverse Velocity-Curve, and measures the (superficial) Discharge past the Transversal.

[This is also true for Curve 4° in a rectangular Cross-Section].

Curve 4°. The volume included between the Cross-Section plane and the bed, banks, and surface, and a vertical plane through the line in question, is equal to the volume of the Velocity-Surface, and measures the Cubic Discharge.

9c. HYDRAULIC ELEMENTS.—The quantities (h, H, b, B, A, R, or as many as seem requisite) for each Curve are figured across it. The lengths of H, R, or of h, H, R (observe that h = H at the Soláni Aqueduct) are also plotted upon a single vertical placed a little to one side of the Curve, and drawn upwards from the Base-Transversals, (on the same scale as the Cross-section).

[The two h, H are plotted on the same side of the vertical in question: the order of the lettering (as h, H; or H, h) indicates the order of magnitude, the lesser being placed nearer the Base-Line].

Soláni Aqueducts. In both these channels the surface-breadth (b) falls short of the full width of the channel (85') in very deep and in very shallow water, (*see* Pl. I, 3, and Pl. II, 4), viz.—

1°, by the amount of the corbelling when the depth exceeds 7'·3.

2°, by the contraction near the bed when the depth falls short of 2'·0.

In all such cases (Pl. XXVI, XXVII, XXIX, XXXI, XXXII), the full width

(85') is shown on the Velocity-Curves by the space between the dotted lines, and the actual surface-breadth (b) is shown (by arrow-marks) on the Base-Transversal, and also figured thereon.

10. *Immersion of Instruments.*—This is indicated when necessary on the Tables and Plates as follows:—

1°. *Surface-Velocities.* Immersion trifling: no indication required.

2°, 3°. *Mid-depth- and Bed-Velocities,* (Tab. XXXII, LVII and Pl. XXIX, XXX). The length (l) of the Connector of the Double-Float, *i.e.*, the nominal depth of immersion (s) of the Sub-Float, is figured in Col. 2 of the Tables, and also shown in the Plates by the lengths of the verticals drawn from the water-surface.

4°. *Mean Velocities* (past any vertical). The practice in this sort of work was to use in each Float-course the longest possible Rod (of the stock* available), so that the Length of Rod was in each Float-course usually only a little shorter than the Average Depth along the Float-course. It is therefore unnecessary to indicate the immersion in detail in the Tables. It is indicated sufficiently in the Plates (XXXI—XLI) by the lengths of the verticals which are the pictorial representations of the Rods: these are everywhere carried down nearly to the bed.

[*Solani Twin Aqueducts.* At these Sites (the bed being pretty level) Rods *of same length* were used with a few exceptions right across. This length (l) is figured both in Col. 2 of Tab. XXXIV—XLI, & LVII, and also on each Velocity-Curve, Pl. XXXI—XXXIII].

11. *Velocity-ordinate Exaggeration.*—The Velocity-ordinates throughout all these Plates are on scale of 2·5 feet per second to an inch, whilst the Cross-sections are on the scales of 25 feet and 10 feet to an inch for the Wide Channels and Distributaries respectively. The Cross-section scales of course determine the scale of the abscissæ, and therefore of the Float-course spacing, on the Base-Transversals.

Scales of spacing and of velocity are of course not really comparable; but it is clear that—adopting the second as the time-unit—the velocity-ordinates are exaggerated 10 times in the wide Channels, and 4 times in the Distributaries. It will be seen therefore at once that all the Curves are very flat except near the banks.

12. **Properties of the Curves.**—A general examination of the Curves (Pl. XXVI—XLI) will show at once the following prominent properties—in addition to those (common to all Velocity-Curves) discussed in Ch. VI, 15—from which it will be remembered that in this discussion Curves derived from numerous Sets of data are entitled to more weight than Curves depending on only a few Sets (in consequence of their better approximation to really Average Curves). It will be convenient to first simply state these properties shortly all together, and afterwards to state the evidence for them. In what follows, Curves on the same Transversal will for shortness be styled "Curves of same kind", or

* i.e., of the Set proceeding by whole feet or half feet, (Ch. XV, 6.)

shortly LIKE CURVES, and Curves on different Transversals will be styled "Curves of different kind", or shortly UNLIKE CURVES.

[The properties detailed are such as may be supposed characteristic of Average Curves, under the particular conditions set forth : it will commonly happen that no single actual Curve will show any one of the properties perfectly ; all that can be expected to be seen in the actual Curves are certain prominent *tendencies* when certain conditions preponderate, (confused of course by the features due to the less dominant conditions)].

i. "The velocity-variation (in any one curve) approximates to the following distribution (in the case of a symmetric cross-section with a level or wholly concave bed, in a long uniform straight Reach), except where modified by the causes named below",..(3).

"the maximum velocity near the centre",..(3*a*).

"a very slow decrease of velocity from the centre towards both banks,". .....(3*b*).

"which becomes more rapid with approach to the banks", ...(3*c*).

"and is very rapid close to the banks",..(3*d*).

"the curve is wholly convex down-stream", ...(3*e*).

"and is symmetric about mid-channel",..(3*f*).

The departures from the above form may be nearly all traced to the two following causes :—

I. Irregularity of the channel above and below the Site,

II. Irregularity of banks and bed at Site,

ii. "Every marked change in the figure of the bed produces in general a marked effect on the figure of the velocity-curve",..(4).

This takes effect as follows :—

"Increase of depth tends to increase of velocity, and *vice versâ*",.............(4*a*).

"The maximum velocity-lines tend to be in the deepest channel (if sufficiently far removed from the banks)",..(4*b*).

"A convexity in the bed causes a concavity in the velocity-curve, and *vice versâ*",..(4*c*).

"These effects are usually more marked in shallow water than in deep water",..(4*d*).

iii. "Velocity at same point of like Curves increases and decreases *cæteris paribus* with rise and fall of water-level", ...(5).

iv. "Like Curves are similar under similar External Conditions",.............(6).

v. "Like Curves of equal mean velocity are *cæteris paribus* equally flat as a whole",..(7).

vi. "Curves of low velocity are *cæteris paribus* flatter than those (of like kind) with high velocity",..(8).

vii. "The Flatness of a Curve as a whole does not depend so much on the general depth of water as on the Mean Velocity, so that Curves at low water or not necessarily flatter than Curves (of like kind) at high water", ...(9).

viii. "Vertical banks give rise *cæteris paribus* to flatter curves than stepped or sloping banks", ...(10).

ix. "The Curves are flatter *cæteris paribus* at the wider Sites",.............(11).

x. "Of unlike Curves the Surface-Curve is the most rounded, the Mid-depth Curve one of the most protuberant, and the Mean Velocity Curve is one of the flattest",..(12).

[The following Articles (12, i—ix) containing the evidence of these properties are numbered (for easy reference) with the same numbers (i—ix) as the properties themselves].

12, i. GENERAL FIGURE.—These properties (8a—f) are best exemplified either by the case of a level bed, or by the case of deep water at Sites with uneven bed (as the unevenness of the bed tells less in deep water), *see* the following :—

Level Bed, (Solání Aqueduct,) Pl. XXVI, XXVII, XXIX—XXXIII.

Uneven Beds, (Deep water,) Pl. XXVIII, XXXIV, XXXV, XXXVII—XL.

After examining the Curves above quoted (which contain the proof of Results 8—8f), the departures (I and II above) from regularity of figure may be discussed.

I. *Channel irregular above and below the Site.* There is a very marked departure from symmetry of figure (about the central ordinate) in many of the curves in both the Solání Aqueducts. This is probably mostly due to the peculiarity of the channels of approach to and exit from the Twin Aqueducts being of about twice the width of the Aqueducts themselves (Pl. II, 3), and to the inadequacy of the length (932') of the Aqueducts to establish complete symmetry of motion within themselves.

CASE 1°. This is very marked in the exceptional case (Pl. XXXIII) of the closure of the Left Aqueduct, when—apparently in consequence of the very unsymmetric arrangement (Pl. II, 3) of the channels of approach and exit—most of the water entering the Right Aqueduct seems to have been *deflected towards the right bank*, displacing the maximum velocity-line also to the right of the centre.

CASE 2°. (Low water). A precisely *similar feature* is also very marked in the case of low water (H < 4') in the Right Aqueduct (Pl. XXXII, Ser. 123—127) due probably to a similar cause, viz., to the presence of the remains of the dams which had been used (Ch. III, 12b, c) to close the Left Aqueduct : these would form an efficient obstruction (of the Left Aqueduct) at low water, causing as before deflection of the water entering the Right Aqueduct towards its right bank.

CASE 3°. (Deep water). In deep water (H > 4') in both Aqueducts the effect is different : the channels of approach and exit being symmetrically placed (Pl. II, 3) with respect to the pair of Aqueducts, the presence of quick water above and below the central pier, seems to *cause more water to pass along* (on either side of) *the central pier* than passes along (near to) the outer banks, so that the velocities near the central pier (in the 30', 32½', 35', 37½', 40' lines) are generally greater than the velocities at equal distances from the outer banks (in the 30', 32½', 35', 37½', 39½' lines). This is well marked throughout Pl. XXVI, XXVII, XXIX, XXX, XXXII, so long as H > 4', the only exception (and that a partial one) being in Ser. 106.

This leads to a fuller rounding of most of the velocity-curves near the central pier than near the outer footways : this effect is possibly enhanced in the case of very deep water (H > 9') by the contraction of waterway near the surface due to the corbelling, (*see* Pl. XXVI, Ser. 53 ; Pl. XXXI, Ser. 101—103, 108—110). It leads also in most of these curves to a *displacement of the higher velocities*, and in many cases of the actual maximum velocity-line towards the central pier.

Thus the Velocity-Curves of either Aqueduct are commonly unsymmetrical curves, but the pair of Curves at equal depths in either channel are taken together tolerably

symmetrical about the central pier, (*see* Pl. XXVI, XXXI in which the curves are arranged in pairs—with nearly same water-level—to exhibit this).

II. *Banks and bed irregular.* The effect due to this is conveniently discussed as part of Property ii, (in next Article).

12, ii. *Velocity follows depth.*—The properties to be discussed are Results Nos. (4—4*d*), *see* Art. 12.

The Velocity-Curve would (by these Results) be apparently wholly convex over a bed that was either level or wholly concave. Any convexity in the bed would cause a corresponding concavity in the velocity-curve; and these effects would be usually more marked in shallow than in deep water (because a variation of depth that is considerable in shallow water is of little importance in deep water).

Three good instances of this, and one remarkable apparent exception (Case 4°, *infra*) occur at the Solání Embankment (Minor and Main) Sites.

CASE 1°. *Central Depression.* In many of the Velocity-Curves (Pl. XXVIII, XXXIV, XXXV, XXXVI) at these Sites there is a slight but extensive depression at and about the centre, giving rise to two lines of maximum velocity at some distance on either side of the centre. This feature is quite marked in the Surface Velocity-Curve (Pl. XXVIII) in deep water, is only slightly marked in the mean Velocity-Curves (Pl. XXXIV, XXXV) at high water, and is very prominent in the Mean Velocity-Curves (Pl. XXXVI) at low water.

This is probably due in part to two causes—

(*a*), to the obstruction of the central Pier of the Solání Aqueduct in the middle of the channel (at a distance of 4769′, 3195′, 2860′ from the Main, Upper Minor, and Lower Minor Site respectively).

(*b*), to the presence of an extensive slight shallow* about mid-channel of each Site.

It seems difficult to separate the effects due to these two causes. The two maximum velocity-lines appear to be at about 40′ on either side of centre, *i.e.*, about opposite to the centres of the Twin Aqueducts, and also over the deepest parts of the Sites themselves, so that so far both causes seem equally efficient. On the other hand the greater prominence of this central depression in the curves at low-water (Pl. XXXVI),—a state in which variations of depth are of course more telling—certainly points to the latter (variation of depth) as at any rate one very efficient cause of the phenomenon. [This confirms Results (4*b*—*d*) of Art. 12].

CASE 2°. *Velocities over top steps.* A further and very remarkable confirmation may be found by comparing the velocities over the top immersed steps *of opposite banks* throughout Ser. 151 to 166 in the Detailed Tables, or (much more readily) in Abstr. Tab. 10, in which an Abstract of these Results is given for ready reference. The velocity-measurements being made over the centre of the steps in question, might be expected *cœteris paribus* to be nearly equal. They are, however, commonly very unequal. The explanation of this is that the Steps on the right bank are all about ·15 of a foot lower than the corresponding Steps on the left bank; so that as the real depth of water over either could not exceed about 9″, and was often very small, the *relative depths* on the two steps were often *very different.*

* From the smallness of the cross-section scale, this feature is not very distinct in Pl. XXVIII, but it will be obvious on referring to the figures at the foot (showing Heights of bed above Datum).

The Table shows that the greater velocity is usually *over that step which is the more deeply immersed.* [This confirms Results (*4a, d*) of Art. 12].

CASE. 3°. *Rapid change of water-level.* In two instances the velocity-work was done (for a special purpose) whilst the water-level was rapidly changing, the whole change (+·75, −·70) being in each case a *considerable fraction* of the general depth, as shown in Table below. The velocity-work was begun in both cases from the left bank, so that the rapid change of water-level caused changes of depth *during the velocity-work* as follows:—

Ser. 164, Increase of depth from left bank to right bank.

Ser. 235, Decrease of depth from left bank to right bank.

The effect of the rapid changes of depth on the figure of the velocity-curve (*see* Table) is well seen in the Plates quoted, and confirms the general Result (*4a*) of Art. 12.

SITE.	Series.	Sets.	Gauge-Reading	Variation of water-level.	Hyd. Mean Depth.	Plate.	Effect of change of water-level on the velocities, [see Plate quoted].
Solání Embankment Main Site, ...	164	1	5·24	+ ·75	5·98	XXXV	High velocities towards right bank.
Míranpur Distributary, ...	235	1	3·75	− ·70	1·63	XLI	Low velocities towards right bank.

CASE 4°. *Depression at Steps.* In most of the Mean Velocity-Curves of Pl. XXXIV, XXXV there is a *strongly marked* indentation at both the 74½′ lines, most marked in the curves in deep water (Pl. XXXIV), gradually decreasing as the water-level falls, and disappearing altogether when fewer than 4 steps are immersed, (Pl. XXXV, Ser. 163 to 166.)

As the Mean Velocity past the 74½′ *vertical* is in many of these cases actually *less than that past the adjoining* 75½′ *vertical*, this is an *apparent exception* to both of the general rules (Results (*4a*) & (*3c*) of Art. 12 above) that velocity decreases in general with decrease of depth, and also with approach to the edge, and therefore merits special attention. The cause of the apparent irregularity is no doubt the presence of comparatively slack water all along the upright face of the 4′ drop-wall, (*see* Cross-Section on either Plate.)

Thus—in the case of deep water—the velocities at all points above lowest step-level on both the 74½′ and 75½′ verticals are comparatively quick, and not very dissimilar—because the higher immersed steps, (which form an important part of the resisting margin) are at a considerable distance from both verticals: whilst the velocities *past all points below lowest step-level* on the 74½′ vertical are *comparatively slow.* So that the Mean Velocity past the 74½′ vertical is actually *less* than that past the 75½′ vertical *in the case of deep water.*

But, as the water-level falls, this state of things alters: the 75½′ vertical being nearer the immersed steps than the (upper part of the) 74½′ vertical, the average velocities past all points of the former are probably always less than past the (upper part of the) latter: and this difference between them would increase as the water-level falls

(because the relative proximity of the immersed steps to the 74½′ vertical becomes more important), so that the comparative slackness of the lower water on the 74½′ vertical decreases in relative importance as the water-level falls.

Thus the anomalous excess of the mean velocity past the 75½′ vertical over that past the 74½′ vertical (which is well marked in deep water) would decrease as the water-level fell, and at last vanish when the water-level fell, so that the mean resistance (from the wetted borders) became equal on each of these verticals. Lastly, at lower water-levels the Mean Velocity past the 75½′ vertical would be in excess (*see* Pl. XXXV, Ser. 163 to 166).

[For further illustration of the effect of the slack water near the 4′ drop-wall, *see* Pl. XVIII, Ser. 44 to 46].

Again, even the Surface Velocity-Curve Pl. XXVIII shows (in a slight degree) certain effects probably due to the abrupt change of figure (viz., the sudden decrease of depth) at 75′ on either side of the centre. The surface-velocities certainly decrease on the whole from the two lines of maximum velocity (about 40′ on either side of centre) outwards, *i.e.*, towards the banks : but the rate of decrease is not continuously increasing (as might be expected) ; the curve shows a slight indentation *at both the* 70′ *lines*, showing the rate of decrease of velocity to be somewhat less between the 70′ and 75′ lines than between the 65′ and 70′ lines. This would seem to be due to the retarding effect of the solid steps, the salient corner of the lowest step of which is actually *nearer to the water surface* in each 70′ line than the bed immediately below that line, causing a somewhat sudden slight increase of resistance which takes effect in a slight abnormal decrease of surface-velocity in the 70′ line.

12, iii. *Velocity follows water-level changes.*—The property to be discussed is Result (5)—

"Velocity at same point of Like Curves increases and decreases *cœteris paribus* with rise or fall of water-level",..(5).

A general decrease of the velocities of each Curve is obvious in passing from high to low water at each Site in all the Plates XXVI—XLI, but it is by no means certain that this is due to the decrease of depth, inasmuch as there are several cases of the contrary so strongly marked, that it is clear that decrease of depth *does not necessarily involve* general decrease of velocity :—

Compare Ser. 123 with 124, 125, 126, (Pl. XXXII.)
Ser. 131, 133 with 132 and with 134 to 139, (Pl. XXXIII.)
Ser. 171, 172 and 178 with the following, (Pl. XXXVI.)

It is therefore possible that the otherwise general decrease in passing from high to low water may be really due to the mode of Control in passing from high to low water.

Velocity over top step. The velocity-measurements made over the *top immersed step* (Art. 6f, 2°, & Pl. XXV) on either bank at the Soláni Embankment Main Site, however bring out the *effect of depth* very clearly.

The depth of water over such step could not exceed about 9″ (the height of each step being about 9″), and was often much less, so that a *small* change of water-level often produced a *great change of relative depth* thereon. The change of velocity corresponding is quite remarkable even within the limits of each Series, and will be found to *follow the change of depth.*

A glance at Tab. XLII—XLV, Ser. 151 to 166, will show at once that the

"Range" of the velocities is on the whole *much greater on the top immersed step* than at any other part of the Channel.

The fact of the velocity-change following the change of depth can also be seen in the Detailed Tables, but is best seen in Abstr. Tab. 10, which shows—for each Series—the following data *for each bank separately*,—

1°, the number of the top immersed step (reckoned from top step),

2°, the number of SETS of velocity-measurements done thereon,

3°, the greatest and least depth of water thereon,

4°, the greatest and least velocity-measurements thereon.

It will be seen that within the limits of each Series there is a pretty steady *decrease of velocity with decrease of depth* (over the same step). In several cases (Ser. 151, 152, 154, 159, 160) the water-surface—being near the level of the tread of a step—falls from one step to another, and therefore from very shallow water on the upper to *comparatively* deep water on the lower step: in each case the sudden increase of depth is accompanied by an *abrupt increase in velocity*.

A similar correspondence (*i. e.*, decrease of velocity with fall of water-level) can also be traced (though ill marked) even within the limits of the same Series over the step next below the top immersed step, but the effect is quite untraceable (within the limits of a Series) at other parts of the channel, being masked (if existent) by the Unsteady Motion: this is to be expected, for the fall of water-level within the limits of a Series (only ·3 of a foot as a maximum) causes a quite trifling relative decrease of depth except in the very shallow parts (*e. g.*, over the top immersed step).

[The advantage of only comparing within the limits of the same Series as above, is that similarity of External Conditions is thus most likely].

12, iv. *Similar Conditions give similar Curves.*—This may be inferred from comparing *successive* (*like*) *curves* at same Site throughout Pl. XXVI—XL, (excluding all cases in which the mean velocities are not nearly equal.) Thus this comparison will include many pairs of curves at nearly same water-level, and with nearly same mean velocity, and therefore presumably under nearly similar External Conditions. A general similarity of shape in such pairs will be obvious, and in running the eye down the whole group of curves at any one Site, a slow change of shape will be obvious in passing from high to low water, or from high to low mean velocity: from which, taken together, it may be inferred that—

"Like Curves are similar under similar External Conditions",..................(6).

12, v. *Equal flatness with equal Mean Velocity.*—This follows from last Article in the case of Like Curves *at same water-level.* But, it will appear that there is also a general similarity of flatness as a whole (not extending to similarity of detail) in Like Curves of equal Mean Velocity, *independently of the state of water-level.*

Owing to the very great amount of Control occasionally exercised, there are several cases available of pairs and trios of Like Curves, at same Site, with nearly equal Mean Velocity, in which the *water-levels are very different.* An Abstract of these is given in Table below. It will be seen (on referring to the Plates) that the Curves of each pair or trio are *of nearly equal flatness on the whole*, notwithstanding the great differences of water-level.

SITE.	PLATES.	Serial Nos.	Number of Sets.	Gauge-Readings, h or H.	Mean Velocities, V.
Solání Right Aqueduct,	XXXI, XXXII	110, 111, 115	20, 16, 1	9·33, 8·97, 7·80	3·83, 3·86, 3·86
	XXXI, XXXII	112, 119	18, 7	8·58, 6·15	3·73, 3·74
	XXXII, XXXIII	126, 131	1, 2	1·92, 4·60	1·24, 1·24
Solání Embankment Main Site,	XXXVI	168, 176	1, 2	3·98, 2·83	1·69, 1·65
		173, 174, 179	5, 1, 4	3·47, 3·04, 1·92	1·35, 1·34, 1·27
		170, 177	2, 4	3·64, 2·42	1·50, 1·50
		171, 178, 180	3, 2, 2	3·62, 2·43, 1·67	·86, ·81, ·87

The inference seems fair that—

"Like Curves with equal mean velocity are *cæteris paribus* equally flat as a whole",..(7).

12, vi. *Flatness of Low velocity Curves.*—The property to be discussed is—

"Curves of low velocity are *cæteris paribus* flatter than those (of like kind) of high velocity",..(8).

Owing to the very great amount of Control occasionally exercised, there are five cases available of pairs of curves (of same kind) at same Site with approximately the same water-level, in which the *Mean Velocities are extremely different.* This permits the effect (upon the figure of the curve) of a general change of velocity throughout the Curve to be studied *apart from other disturbing causes.* An Abstract of these cases is given in the Table below. The property in question will be sufficiently obvious from consulting the curves there referred to.

SOLANI SITE.	PLATE.	Serial Nos.	Number of Sets.	Gauge-Readings. h or H	Hyd. Mean Depths. R	Mean Velocities V	Ratio of Mean Velocities in curves compared.
Right Aqueduct,.. ..	XXXII	124, 123	1, 1	3·49, 3·65	3·26, 3·40	2·43, ·71	3·42 : 1
Right Aqueduct,.. .. [Left Aqueduct closed].	XXXIII	134, 133	1, 1	3·58, 3·60	3·33, 3·35	3·22, ·69	4·67 : 1
Embankment Main Site,	XXXVI	170, 171	2, 3	3·64, 3·62	4·73, 4·72	1·50, ·86	1·74 : 1
	XXXVI	170, 172	2, 2	3·64, 3·58	4·73, 4·68	1·50, ·66	2·27 : 1
	XXXVI	177, 178	4, 2	2·42, 2·43	3·64, 3·64	1·50, ·81	1·85 : 1

The same general property may also be at once seen by glancing down the whole group of Curves of same kind for any one Site. It will be seen that in passing from high to low water there is on the whole a gradual general decrease of velocity, and that this is *accompanied in general by increased flatness of the curves.* This is of course most marked at those Sites for which the range of velocity is greatest.

[Compare especially Pl. XXXI, XXXII; also Pl. XXXIV, XXXV, XXXVI].

It will be seen also that in those occasional cases where the velocity is unusually high throughout the curve, the curve is in each case much more fully rounded than the curves with low velocity at neighboring water-levels.

[Compare Ser. 131, 132, 133, Pl. XXXIII.]

12, vii. *Flatness not dependent on depth.*—The gradual flattening of the Curves of any one kind at the same Site in passing from high to low water is so marked, that it would at first sight appear to be a *general property* that Low-water curves are flatter than High water curves (of the same kind). This is, however, not the case: there are in fact several cases of the contrary, viz., of curves which are *much flatter* than several of like kind in shallower water, as shown below, (*see* also Plates quoted):—

PLATE.	SELECTED CURVE.			Selected Curve flatter than Curves named below, all in shallower water. (see Plates quoted).		
	Serial Nos.	Gauge-Readings.	Mean Velocities.	Serial Nos.	Gauge-Readings.	Mean Velocities.
XXXII	123	3·65	·71	124, 125, 126	3·49, 2·02, 1·92	2·43, 1·61, 1·24
XXXIII	131	4·60	1·24	132, 134—139	3·96, 3·58—2·66	4·83, 3·22—2·20
XXXIII	133	3·60	·69	134—139	3·58—2·66	3·22—2·20
XXXVI	171, 172	3·62, 3·58	·86, ·66	173—177	3·47—2·42	1·35—1·50
XXXVI	178	2·43	·81	179	1·92	1·27

but it will be seen that all the flat curves are cases of Low Velocity Curves.

Again, the seven cases quoted in Art. 12, v of pairs and trios of Curves of nearly same mean velocity, but with very different general depths, and nevertheless of nearly equal flatness on the whole, point to the same Conclusion that—

"The Flatness of a Curve as a whole does not depend so much on the general *depth* of water as on the Mean Velocity, so that Curves at low water are not necessarily flatter than Curves (of like kind) at high water", ...(9).

[The gradual flattening of the Curves of any one kind in passing from high to low water (above alluded to) appears in fact to be really owing to the accident that the ordinary Control of the Canal was such that there was usually a gradual decrease of the general velocity in passing from high to low water].

12, viii. *Vertical and sloping banks.*—The general effect of the banks is that—

"The decrease of velocity near the banks is greater at the same relative distance from the edge in the case of sloping or stepped banks than of vertical banks ",(10*a*).

This can be at once seen by comparing the marginal portions of the curves at the Solání Embankment Main and Fifteenth Mile Sites (Pl. XXXIV—XXXVII) with those of the curves for the Solání Aqueduct (Pl. XXXI, XXXII). This is no doubt due to the gradual decrease of depth near the edge in the former case, and forms a particular instance of the effect of decrease of depth in decreasing velocity (Result (4*a*)). This may be expressed as follows:—

"Sloping or stepped banks give rise to curves which are sharply rounded over the banks",..(10*b*).

"Vertical banks lead to curves which are fully rounded near the banks",...(10*c*), and this leads also directly to the following:—

"Vertical Banks lead to flatter curves than sloping or stepped banks",......(10).

This can be seen by comparing the curves at the Solání Aqueduct Sites with those at any of the other Sites; but as width is an independent and most important element in determining the flatness of a curve, the comparison will be fairest between the curves at the Solání Aqueduct Sites (Pl. XXXI, XXXII), with those of same kind at the Kamhera Site (Pl. XL), which is (roughly speaking) of similar width.

12, ix. *Wide Sites give flat Curves.*—The general effect of Width of Site is—

"At different Sites of similar character Like Curves are—each taken as a whole—flatter throughout at the wider Site", ... (11).

This is well seen by comparing the Curves figured for the Belra, Jaolí, Kamhera, and Distributary Sites, (Pl. XXXVIII—XLI,) all in earthen channels, and of roughly similar (trapezoidal) cross-section, but *varying extremely in width*, as shown in Abstract below.

SITE.	PLATE.	Serial Nos.	Bed-width.	Surface-widths. b	Hyd. Mean Depths. R	Mean Velocities V	Rough Comparison of Sites.	Comparison of Curves.
Belra,	XXXVIII	201-206	180	188·4-186·8	9·02-7·60	3·17-2·92	Very similar.	Very similar, flatter than all below.
Jaolí,	XXXIX	211-217	185	192·8-190·9	7·82-6·82	2·96-2·63		
Kamhera,	XL	221-225	50	65·5- 64·0	4·84-4·07	2·86-2·71	Surf.-width ⅓ of above	Flatter than all below.
Right Jaolí	XLI	231, 232	16	25·0- 22·0	2·79-1·99	2·52-2·10	Surf.-width ⅓ of last.	Flatter than all below except Mírán-pur.
Míránpur,	XLI	236	10	18·5	1·16	1·49	Surf.-width ½ of last.	Flatter than all below.
Mansúrpur	XLI	233, 234	8	14·1- 13·8	2·32-2·10	2·10-2·05		Very similar.
Pimora,	XLI	237, 238	6	13·0- 12·5	2·16-1·94	1·83-1·75		

It will be seen that—commencing from the foot of the Table—the Curves increase steadily in flatness with increase of width of the Site, (with one exception,) notwithstanding that the increased depth and increased velocity, which chance (in these particular instances) to accompany the increase of width, would each separately tend to diminish the flatness of the Curves. In fact, the increase of flatness attendant on increase of width is by no means fully seen in these Plates, being partly compensated by the increase of depth and velocity.

[In comparing these Curves, it must be remembered that the scale of abscissæ in Pl. XLI is 2½ times as large as that used in Pl. XXXVIII—XL, so that the Curves on Pl. XLI are thus made to *appear* relatively much too flat. Were the same scale used throughout the relative flatness of the Curves on Pl. XXXVIII—XL, would be seen to be much greater compared with those on Pl. XL than is now apparent to the eye. Again, the single exception of the Míránpur curve (No. 236) being flatter than the curves Nos. 231, 232 at the wider (Right Jaolí) Site is sufficiently explained as due to the higher velocities in the latter, which mask (Art. 12, vi) the effects of the difference of width].

12, x. *Comparison of Unlike Curves.*—The general properties proposed are—

"Of Unlike Curves under same External Conditions at the same Site (of rectangular section), the Mid-depth Curve is usually the outer (except near the centre), the Mean Velocity Curve intermediate, and the Bed Curve the inner",...... ..... (12*a*).

"The Mean Velocity Curve is one of the flattest, and the Surface Curve the most fully rounded, (so much so that near the banks the Surface-Curve is one of the innermost)", ... (12*b*).

COMPARISON 1°, (*Simultaneous Field-work*). In order that such curves might be fairly intercomparable in all respects, it is necessary that the velocity-ordinates to be compared should be derived *from velocity-measurements made nearly at one time*, so as to secure the certainty of similar External Conditions. The Field-work of each Set of Series 241, 242, 243, including Surface-, Bed-, and Mean Velocity work, was specially done with this view as follows:—

Field-work of 1°. In each Float-Course one Instrument of each kind—viz., one Surface-Float, one Double-Float (with Sub-Float near the bed), and one Mean Velocity Rod (reaching near to the bed)—was cast in as rapid succession as possible from the Upper Boat: after these a second Instrument of each kind, and then a third, a fourth, &c., until three good observations of each kind had been secured in that Float-Course. After which the work was taken up in the next Float-Course in succession, and so on. In this way a SET of velocity-measurements of each kind was obtained, executed to the minutest detail with the nearest approach to similarity of External Conditions throughout that is practically attainable.

Unfortunately the time occupied in obtaining a single complete SET of each kind was so great (about 4 hours) as to be quite prohibitory. Only two complete Sets of each kind were obtained. These are grouped specially in Tab. LVII into Series (one of each kind): from the way in which the Field-work was done (as above), the various External Conditions (Depth at Gauge, Surface-breadth, Fall of Water Surface, Wind) are of course the same for the three SETS (one of each kind) of each day.

The Velocity-Curves resulting are shown plotted upon the same Base-Transversal in Pl. XXX, *Fig.* 3, and obviously bear out the general statements above. Some irregularity in the Curves must of course be expected from the small number of Sets (only two) on which they depend.

COMPARISON 2°, (*Non-Simultaneous Work*). Further evidence tending to the same Conclusions may be obtained from *Fig.* 1, 2 of Pl. XXX, each of which contain four Transverse Curves (viz., Surface, Mid-depth, Bed, and Mean) *under as nearly similar conditions as possible* (as shown in Abstract below,) selected from the Detailed Tables, all plotted upon the same Base Transversal.

Figure.	CURVE.	Serial No.	Sets.	YEAR.	DEPTH. H	DEPTH. Varn.	SURF. WIDTH. b	Length of Instrument.	SURFACE-FALL. F_1	SURFACE-FALL. F_2	WIND. D	WIND. V	Mean Velocy. U or V
Fig. 1.	Surface, ..	56	16	1875	8·71	·15	83·9	..	5·78	4·71	SSW	2	3·79
	Mid-depth, ..	62	1	1876	9 05	..	83·5	$4\frac{1}{2}$	5 80	4·70	SW *b* S	11	4·13
	Bed, ..	66	4	1876	8·77	·30	83·9	8	5·81	4·85	SW *b* W	5	3·47
	Mean, ..	111	16	1876-7-8	8·97	·16	83·7	8 3	5·91	4·87	N *b* W	1	3·86
Fig. 2.	Surface, ..	53	16	1878	9·90	·13	82·0	..	6·04	5·44	NE	2	4·33
	Mid-depth, ..	61	16	1877	10·00	·24	82·0	5	5·93	5·57	NE	2	4·29
	Bed, ..	65	3	1877	10·00	·00	82·0	10	6·08	5 52	NE *b* E	4	4·21
	Mean, ..	108	19	1876-7-8	9·96	·19	82·0	9·3	5·85	5·61	NE *b* E	1	4·00

In comparing these Curves, it must be remembered that the Mid-depth-Curve No. 62, which contains only one Set, is of course irregular in detail: also that the two Bed-

Curves Nos. 65, 66, containing only 3 and 4 Sets respectively, are also naturally less regular than the remaining Curves, which contain 16 or more Sets in each. The evidence from these Curves is not so good as that from those first described (of *Fig.* 3), in consequence of their Field-work not having been executed in concert. In particular the Mean Velocity Curve of *Fig.* 2 lies within all the rest, a state which seems almost impossible in the case of Curves really under the same External Conditions.

[The External Conditions so far as registered in the Table above, do not seem to differ sufficiently to account for this anomaly. It may be accounted for by supposing that the Instruments used in either Ser. 65 or 108 were in imperfect adjustment, a supposition however for which there are no other grounds; or again by supposing that the External Conditions were not sufficiently similar. This shows the desirability of the Field-work of Curves to be so compared being executed in concert in the way explained for *Fig.* 3].

The same general Conclusions as to the relative positions of the different Transverse Curves may also be drawn from the Vertical Curves figured for the same Site in Pl. XIII, XVI, XVII, which make quite clear the general facts of the general prominence of the Mid-depth Curve throughout (and especially near the banks, *see* Pl. XVI), of the intermediate position of the Mean Velocity Curve, and inclusion of the Bed-Curve within the rest; and, lastly, of the great decrease of the surface velocity-curve near the margin.

[These Conclusions agree throughout with the Results of the Bazin Experiments on Rectangular Open Channels, (*see* the "Atlas" thereof, Pl. XVIII, *Fig.* 8). The Velocity-Curves are unfortunately not drawn (except in the one case quoted) in the Atlas; *only cross-sections* of the experimental channels being given with relative velocities (at a series of points on several verticals and transversals) figured on them, and with a system of curves (similar to contours) showing the loci of points of equal velocity drawn within them: the whole of the above Conclusions can, however, be drawn from an attentive consideration of these quasi contour-curves on Pl. XIX, XX, XXI, of the Atlas].

13. Velocity-Surface.—A good idea of the form of the Average Velocity-Surface in an open rectangular channel may now be formed from the consideration of both systems of Average Velocity-Curves (Vertical and Transverse) with help of the following physical illustration.

Conceive a thin membrane stretching across such a channel at a very short distance below (on down-stream side of) the cross-section plane, and bulging out from it down-stream so as to form a sort of domed surface everywhere concave to the Base-plane (or convex down-stream), and symmetric about the central vertical plane-section.

The maximum velocity-ordinate would lie in that central section above ½ of the depth, and the depth of the maximum velocity-ordinate of a longitudinal vertical section would increase from the centre towards the banks where it would be about mid-depth.

The Mid-depth-Curve would be one of the most protuberant and yet one of the flattest, and the Bed-Curve the least protuberant. The Surface-Curve would be one of the most rounded, though not the most protuberant.

Next conceive the space between this membrane and the Base-plane filled with water. The volume of water would be the Cubic Discharge (per second).

Lastly, conceive the membrane itself to be very flexible but inextensible, and thrown into a system of violent irregular rapid undulations in such a way as scarcely to disturb the level of the contained water. It would now show the figure of the Instantaneous Velocity-Surface, every velocity-ordinate of which is in a state of *constant rapid periodic change* with certain mean values, which are the Average-Velocities, whilst at the same time the contained volume (representing the Cubic Discharge) remains sensibly constant.

14. **Edge-velocity.**—The forward velocity *at the edge* itself is not a quantity of much practical interest, as it is too small to sensibly affect Discharge-measurements. It is, however, of great importance in the Theory of Fluid Motion : unfortunately it cannot be measured by direct Experiment by any known method, and can therefore only be inferred indirectly from the shape of the Transverse Velocity-Curves near the edge. Unfortunately also velocity-measurements near the edge increase in difficulty (with all Instruments alike)—even with long straight banks—with approach to the edge, so that the approximation to the velocity at the edge is a poor one at best.

[Both Floats and Current-Meters are used at great disadvantage very close to the edge even with long straight banks. The shortest distance from the edge at which systematic velocity-measurements were made in the present Experiments was about 7″, and that only in the case of long straight banks as at the Soláni Embankment and Aqueduct Sites,—*see* any of the Tables or Plates of Velocities past a Transversal].

All the Transverse Velocity-Curves in which velocity-measurements were made pretty close to the edge, (and which include a sufficient number of Sets to give good Average Velocities,) agree in showing that—

"The forward velocity near the edge decreases rapidly with approach to the edge",..(18).

But this comes out better (as far as the surface is concerned) in the following :—

14a. Experiment.—It has been explained (Ch. IV, 23) that the principal difficulty in the use of Floats near the edge is the tendency to "deviation" from "fair course". Now this tendency is found to increase with approach to the edge, and in the case of surface-water at any rate it was found to assume the form of a *persistent deviation from the edge towards the centre*, even with long straight banks. This was brought out very clearly by some special Experiments made at two Sites very favorable for the Experiment, *i.e.*, with straight uniform banks of great length, viz.—

1°, within the Soláni Embankment, (Pl. II, 2,) close to the waters' edge.

2°, in the Soláni Aqueduct, (Pl. II, 4,) close to the central pier.

At both these Sites the flow of water along the edge is as quiet as seems possible in the case of a large body of water. Very small Surface-Floats, (bits of cork, tiny chips of wood and straw) were put in to the water, some very near the edge, some touching the edge. As a general rule these tiny Floats moved—

1°, only slowly forward when near the edge.

2°, with the slowest forward motion when nearest the edge.

3°, and sometimes remained almost stationary for a perceptible interval when actually touching the edge.

4°, seldom moved parallel to the edge, but with a marked deviation from the edge towards the centre.

5°, and this deviation from the edge was most marked (or most rapid) when nearest the edge.

[This deviation from the edge was so constant as to render surface velocity-measurement very difficult and uncertain at about 7″ from the edge, and impossible within that distance, (even with a Run of only 10′ length). It was not uncommon, for instance, to have to pass 100 Surface-Floats before the requisite number (three) "good" Floats were obtained at the 41′$\frac{1}{2}$ (from centre) vertical—(only 7$\frac{1}{2}$″ from the edge)—of the Soláni Aqueduct, (*see* Pl. XVII.)

14b. CONCLUSIONS.—Summing up, then, it may be inferred that—

"Near the edge the forward surface velocity decreases very rapidly with approach to the edge", ...(13*a*),

"and is very small (perhaps zero) at the edge", ...(13*b*),

also "near the edge there is a persistent flow (of the water at and near the surface) from the edge towards the centre", ...(13*c*).

"and this action is most intense nearest to the edge, and decreases rapidly with the distance from the edge", ...(13*d*).

[No such persistent action (deviation constantly in one direction) was remarked in the use of the Double-Floats and loaded Rods. It is obvious of course that the persistent flow of the water at and near the surface from the banks must be accompanied by a *contrary flow* (towards the banks) at some other level, so as to preserve the water-level near the edge; but the Experiments did not reveal this. The remarkably steady forward motion of the Rods has already been remarked, (Ch. XV, 9,) and was to be expected].

The transverse motion of the surface-water has been dwelt on at some length, as it affords interesting experimental confirmation of certain theoretical results, *e. g.*—

1°, *see* the passages quoted (Ch. VIII, 2) from Moseley's "Steady Flow", showing that the decrease of pressure over the surface from the banks to the centre (due to the increase of velocity), must necessarily cause such a transverse flow.

2°, *see* also Prof. Jas. Thomson's exposition of the cause of the Depression of the maximum velocity line, in Procgs. of Royal Socy., Vol. XXVIII of 1878-9 in which the Experiment of Art. 14a above is quoted (pp. 125, 126).

14c. *Edge-Velocity not plotted.*—In the uncertainty as to the real magnitude of

the forward velocity at the edge, it being known only to be very* small (Result (13) above), it has been thought better not to show it all in the Diagrams of Transverse Velocity-Curves: the Curves are in all cases *broken off* at the velocity-ordinate next to the edge.

[In the 1874-5 Report these Curves were drawn as continuous lines from the two *ends* of the Base-Line, thus indicating that the Edge-velocity was actually zero: the present practice is now thought better, as it avoids any assumptions].

14d. *Edge-Velocity taken zero in Discharge-computation.*—But, in computing the superficial Discharge past a Transversal, and also the Cubic Discharge, it was of course generally necessary to assign some definite value to the velocity in question. It was accordingly assumed zero for the purpose of Discharge-computation. This assumption was thought justifiable *for this purpose*, because the mode of computation is such that the Result (Discharge) is only very slightly affected by the actual value of that velocity when small, whilst the simplification of the labor of computation resulting is considerable.

This is indicated in the Detailed Tables (XXIX—LVII) by the insertion of the phrase—

"Assumed zero in computing Discharge",

or some similar phrase in the two velocity Sub-columns which should contain the Edge-velocities: where the space is very much contracted, the symbol (?0) is entered, and may be looked on as a contraction for, and as equivalent to, the above phrase, as well as indicating that the Edge-velocity though unknown is certainly very small.

15. *Curve equally good nearly throughout.*—In consequence of the Instruments used for the velocity-measurements past any one Transversal being nearly equally immersed at all parts of the stream, they act with sensibly equal efficiency throughout the whole width of the stream except near the banks, (where the motion of the water is more irregular,) so that—

"The figure of a Transverse Velocity-Curve can be determined with equal precision at all parts except near the edge, where the precision attainable decreases with approach to the edge", ... (14).

[This is a considerable contrast to the case of the Vertical Velocity-Curves, in which it appears (Ch. X, 11) that the approximation obtained decreases steadily towards the bed].

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* The rapid decrease of velocity *near the edge*, and consequent smallness of the Edge-velocity gives rise to the surmise that there is perhaps really a similar rapid decrease of velocity *near the bed* (far more rapid than appears in the Vertical Velocity-Curve Diagrams) with consequent small Bed-velocity (possibly far smaller than that computed in Ch. XI, 13), which has passed unrecognized solely through the imperfection of the means of velocity-measurement close to the bed.

CHAPTER XVIII.

TRANSVERSE CURVES—GEOMETRIC FIGURE.

Preface.—The subject of this Chapter—the Figure of the Transverse Velocity-Curves—is solely of theoretic interest.

1. Transverse Velocity-Curve, GEOMETRIC FIGURE.—The investigation of the geometric figure of the Transverse Velocity-Curves is a very delicate inquiry, but at the same time one of the highest theoretical interest. The same remark—as made in the case of the Vertical Curves, (Ch. XI, 1)—about the unsuitability of the primary observation-data, viz., the velocity-measurements themselves, applies in this case also, and for the same reasons as there given, *q. v.* It applies with greater force indeed to these Curves throughout most of their width, on account of their excessive flatness in the wide central region: but with less force to them *taken as a whole* than to the Vertical Curves, because the decrease of velocity from centre to banks (upon which the total curvature depends) is always a comparatively large quantity, and therefore admitting of fairly approximate determination from the velocity-measurements themselves.

The more salient properties of the Curves—such as could be gathered from a cursory examination—have been already discussed, (Ch. XVII, 12—12, x.) In this Chapter it is proposed to discuss such properties as require numerical calculation in evidence.

2. PROPORTIONALITY OF CORRESPONDING VELOCITIES.—The most prominent of these properties is the following:—

"In Curves of like kind with same water-level at same Site, the velocities at same points are approximately proportional",..(1).

The amount of control occasionally exercised was so great, that there are five pairs of Curves available well suited to bring this out, viz., Curves of like kind with same general depth of water at same Site, but *with extremely different general velocities.* The details are shown in Abstr. Tab. 12, the pairs to be compared being grouped together.

The actual velocities of each Curve are given (in old face type as 2·60, ·72), and

also—to facilitate the comparison—what may be called "reduced velocities" of one of them (viz., of the Curve of higher general velocity, in old brevier type, as ·76), *i.e.*, velocities reduced in the ratio of the Mean Velocities of the two Curves: these "reduced velocities" of the one are the figures to be compared with the actuals of the other; their differences are given in the last line of each pair.

It will be seen that the Mean Velocities of each pair are thus reduced to equality, and that the velocities throughout are also *reduced to approximate equality.*

This establishes the Proposition. This may be expressed also in following form:—

"Curves of like kind with same depth of water at same Site, but with different general velocities are approximately parallel projections of one another",.....(1a).

From this it follows that in attempting to form equations of these connecting the velocity (v) with its abscissa (y), with the depth under it (H_y), &c., the variable Velocity-ordinate (v) should be divided by some one principal velocity, say the central velocity (v_0) or mean velocity (U) of the curve, *i.e.*, should appear in the form—

$v \div v_0$, or $v \div U = f(y, H_y, S, \&c.,)$..........................(2).

It may seem that this is a small number of instances (only five) from which to establish so general a proposition: especially as some of the discrepancies are rather large, *e. g.*, those exceeding ·08 are 8 in number, viz.—

2 of ·10, 2 of ·11, 1 of ·13, 2 of ·16, 1 of ·22,

but these are entirely in the worst determined (the two upper) pairs containing only one Set in each curve. The discrepancies in the better determined (the three lower) pairs are all small, quite as small as could be expected from the small number of Sets contained in each curve. What makes these curves good evidence is the very great differences in the general velocities of the selected pair, the ratios of the mean velocities being 3·42, 4·67, 1·74, 2·27, 1·85 to 1 respectively.

[A great contrast will be seen here between these Curves and the Vertical Curves, in that in Vertical Curves with same depth of water on the same vertical, but with different general velocity, the velocities at the same point are not necessarily proportional: this is clear from the variability of position of the axis of the velocity-parabola. Compare Ser. 8, 9; 22, 23; 27, 28; 32, 33, Pl. XIII, XV, XVI, which are pairs of curves with nearly the same depth on same vertical, but with axis at different depths].

3. Figures proposed.—The figure of the Transverse Curves does not seem to have excited so much interest as that of the Vertical Curves; the Author has only been able to trace the following:—

Work.	Transversal.	Figure proposed.
Boileau's Expts., p. 318, ...	Surface.	Parabola, except near the banks.
Mississippi Report, p. 237, ...	5' below surface.	Parabola at Columbus.
	Surface.	Two half parabolas (sometimes).
Lake River Report, of 1868, p. 961,	[Not stated].	Ellipse.

It is clear that any geometric form of Curve proposed must necessarily possess the properties detailed in Ch. XVII, 12, as the more obvious properties, viz.—

"Excluding the effect of variation of depth (through the cross-section) for the present, the ordinates of the Curve should vary as follows :—

$$\left.\begin{array}{l}(a),\ \text{“ the maximum ordinate at the middle ”},\\ (b),\ \text{“ a very slow decrease from the centre outwards ”},\\ (c),\ \text{“ becoming more rapid near the banks ”},\\ (d),\ \text{“ and very rapid close to the banks ”},\\ (e),\ \text{“ with convexity down-stream ”},\\ (f),\ \text{“ and with symmetry about mid-channel ”},\end{array}\right\}\ldots\ldots\ldots\ldots(3).$$

No curve not possessing these properties would be worth examination.

[The parabola certainly does not possess the properties (*b*), (*c*), (*d*). A mere glance at Pl. XXVI—XLI will show how ill a parabola would fit the whole Curve throughout].

It should be noted that the Curves are so excessively flat throughout the greater part of their width, that almost any geometric curve (the parabola included) could be made to fit the Curve through this region, (*i.e.*, except near the banks). This shows the great importance of the velocity-measurements very close to the banks in this inquiry, as it is *chiefly from them* that the suitability or unsuitability of any proposed figure is ultimately determined.

Three types of Curve, viz., (*a*), Parabolic; (*b*), Elliptic; (*c*), Exponential; will be now discussed. In this discussion (Art. 3a—c) note that—

Length of Base-Transversal $= 2b$, from $y = -b$ to $y = +b$.

Abscissæ (y) are reckoned of same sign as b, so that $y \div b$ is always $+$.

3a. *Parabolic Curves.*—Of all geometric Curves those most easily calculated are those of the Parabolic class (so called from their resemblance to a common parabola) represented by the equation—

$$\frac{v}{v_0} = 1 - m\left(\frac{y}{b}\right)^n \ldots\ldots\ldots\ldots\ldots\ldots(4),$$

and would therefore naturally be the first selected for trial, (and indeed have been suggested as above.) But they are not really suitable Curves. To render this clear, a number of them have been plotted—from (computed) ordinates given in Tab. 19 —in the Diagram at foot of Pl. XLII for the case of $m = 1$ with the following values of n, viz., $n = 1, \frac{3}{2}, 2, 3, 4, 5, 6, 8$, (the case $n = 2$ is that of the common parabola.) It will be seen at once (from the Diagram) that no curve of this class can be said to be much like the actual velocity-curves : those involving low values of n fail altogether in Conditions (*b*), (*d*); whilst those involving high values of n fail in Condition (*d*).

3b. *Elliptic Curves.*—The most easily calculated family of curves which possess the properties required, especially (*b*), (*c*), (*d*), are the Elliptic Curves (so called from their resemblance to an ellipse) represented by the equation—

$$\left(\frac{v}{v_0}\right)^n + m\left(\frac{y}{b}\right)^n = 1, \qquad (5).$$

To render this clear, several of these Curves are shown—plotted from (computed) ordinates published in Abstr. Tab. 19—in the Diagram at middle of Pl. XLII for the case $m = 1$, with the following values of n, viz., $n = 1, 2, 3, 4, 6$. A general resemblance between these Curves and many of the more regular of the actual Velocity-Curves will be at once apparent. The approximation could of course be tested only by actual numerical comparison in each case.

As a matter of fact, the "quartic ellipse" (being the particular case of above when $m = 1$, and $n = 4$) does closely fit several of the Surface Velocity-Curves, especially those of Ser. 51, 52, 56, Pl. XXVI. This was the geometric Curve proposed in the 1874-75 Report (Art. 33) when these three Series were the only data available. On computing this Curve for the whole of the nine Series (Nos. 51—59) now available, it was however found that no single Curve of the Class would properly satisfy all the Observation-Curves, but that the value of n *must gradually increase as the depth decreased*, so as to yield curves increasing in flatness as the depth decreased.

The gradual increase of flatness with fall of water-level as a general Rule has already been noticed in Ch. XVII, 12, vi & vii, as *probably* really due to the decrease of general velocity which usually accompanies fall of water-level: the increase of flatness due to this cause is sufficiently provided for in the proposed equation (5), which gives $v = v_0 \sqrt[n]{1 - m\left(\frac{y}{b}\right)^n}$; this provides that for same values of $y \div b$, v shall be proportional to the central velocity, so that curves drawn upon the same transversal ($2b$ constant) increase in flatness with decrease of the central velocity. But the increase of flatness above meant is of a *higher degree* than this, involving the passing *from one order of curve to another*, such a change in fact as is expressed in a gradual change in the index n.

The irregularity of many of the Soláni Aqueduct Observation-Curves near the edge is, however, so great (caused partly no doubt by the obstruction of the corbelled footway, and partly by the division of the channel by the Central Pier only 932′ long, *see* Pl. II, 3 & 4), that it seems obvious that the equation of the curve should contain in it terms to express the effect produced by the corbelling, and further that a geometric Curve suited to a uniform channel of great length is not fairly applicable to this Site. The inquiry did not seem sufficiently hopeful to be worth continuing the laborious numerical trials under such complex conditions.

[From the experience gained in these attempts, it appears to the Author that by making m, n, and also b variable, the curves of this family would fit very closely to the actual curves in the case of a *uniform rectangular channel of very great length*. The Soláni Aqueduct does not approximate sufficiently to this condition. The labor of such numerical trials would be very great, especially if the Most Probable Curves were computed, as of course they should be].

3c. *Exponential Curves*, (Abstr. Tab. 19, and Pl. XLII).—This general name may be taken to include all Curves involving the abscissa ($\pm y$) in an exponential form. They are interesting in physical inquiries as being the forms resulting from the solution of the *simplest* differential equations, which are themselves the primary

mathematical expression of physical laws. Three of the simplest—the Logarithmic, Catenary, and Error-Curve—have been plotted in the upper Diagram of Pl. XLII from computed ordinates published in Abstr. Tab. 19.

The simplest of these—the Logarithmic Curve, ($v = mv_0 \,.\, \varepsilon^{-y \div b}$)—is given here as being the curve deduced from a certain theory of fluid friction inMoseley's* "Steady Flow" for both the Vertical and Transverse Velocity-Curves in a Pipe flowing full. It is supported by comparing the numerical Results given by it with Darcy's Experiments on the figure of the Vertical Velocity-Curve in Pipes flowing full. In Darcy's Work† a curve of the parabolic class $v = v_0 \,.\, \left(1 - m\left(\frac{y}{b}\right)^{\frac{3}{2}}\right)$ is deduced from the same data. This case is given here chiefly as an illustration of the imperfection of such numerical Tests when the data are unsuited to the purpose. Thus, Darcy's velocity-measurements were executed only at five points of the central vertical, viz., at the centre, and at points above and below the centre at distances of $\frac{1}{3}$ and $\frac{2}{3}$ of the radius from the centre, thus embracing only the central $\frac{2}{3}$ of the vertical diameter *within which the curve is very flat*, and omitting altogether the outer—and for the present purpose most important—$\frac{1}{3}$ of the radius nearest the walls of the pipe. In consequence of the great flatness of the curve throughout the central $\frac{2}{3}$-space, the numerical discrepancies between the ordinates of either trial curve and the velocity-ordinates are of course small; and this is relied on as an experimental verification of the trial curves.

But it is in fact a very poor Test. For the curves proposed are extremely dissimilar in figure; their convexities being actually *turned opposite ways* (*see* Pl. XLII). Darcy's proposed curve is at any rate convex down-stream, (*see* the curve in which $n = \frac{3}{2}$ of the parabolic class,) whereas the Logarithmic Curve proposed is actually *concave down-stream* with a cusp at the centre. This shows the absolute necessity of velocity-measurements close to the margins.

The other two curves of this class (the Catenary and Error-Curve) are given solely as being tolerably familiar and easily calculable curves of this class likely to result from mathematical investigations on this subject; and to show their unsuitability (for use as Trial Transverse Curves) in that they do not properly satisfy Condition (*d*).

[The Catenary was chosen simply because some of the Velocity-Curves do in some respects resemble the figure of a free heavy chain (Catenary); moreover, if a rope of material light enough to float on water be very loosely stretched across the surface of a running stream, it at once assumes what may in a general sense be termed a Catenarian form, the figure of which is in some specific way related to the Surface Velocity-Curve, which in a certain way it also resembles.

The Error-Curve was chosen as being a simple curve of the family denoted by Eq. (48) of Moseley's "Steady Flow", Philos. Maga., Vol. XLIV, p. 35].

It will be seen (*see* Diagram) that all three of these curves fail in giving a sufficiently rapid decrease of the ordinates near the banks.

4. Water-level Change.—As a general summary of what precedes, it appears that—

"Curves of like kind change in shape with change of water-level",..........(6),

* Philos. Maga., Vol. XLII, p. 192, Eq. 12. † Darcy Expts., p. 128.

and that—

"If $v \div V = f(y^n)$ be the general form of their Equation, the exponent n will be found to vary with the water-level, and therefore with the general depth, and may perhaps be expressible as a function of the hydraulic mean depth (R)",...(7).

5. Transverse Curves, General Conclusions.—Viewing the evidence of all the Transverse Curves figured in this Work, together with those in other Works as a whole, the following general Conclusions may be drawn :—

"The figure of the Transverse Velocity-Curves is for given External Conditions determined by the figure of the bed",..(8).

"Convexities of the bed produce concavities in the curve and *vice versâ*", ...(9).

This points to the Conclusion that in the equation of a Transverse Curve—

"The velocity (v) should be expressed as a function not of the abscissa (y) only, but also of the depth (z), so that the equation should be of form—

$v \div V = f(y, z, \&c.)$",..................................(10).

This being the case, the attempt to discover the form of the function by numerical trials with velocity-ordinates given by Experiment is probably hopeless in any case in which the depth is irregular, as in a natural channel.

[The only hope of effecting this *solely from Experiment* (*i.e.*, in the absence of any indication of the proper functional form from a rational Theory) would be in the accumulation of Experiment in specially prepared uniform rectangular and semi-circular channels of very great length compared to their sectional dimensions. In the absence of such special Experiment the attempt would be hopeless. To a certain extent, *i.e.*, on the small scale, this has already been done in Mr. Bazin's Experiments].

6. Velocity-function.—The whole of the preceding discussion (Ch. XVII, XVIII), especially that upon the apparent irregularities of the Curves, and that on the depression of the line of maximum velocity (Ch. XII), is entirely consonant with Dubuat's principle that—

"The wetted border of the channel is the ultimate source of the retardation of flow",..(11),

and that in this term "wetted border" is to be included the layer of air resting on the water-surface, as in some degree aiding in the retardation.

With this view the Author advances the following proposition, viz., that in the algebraic expression for the velocity at a point the velocity should perhaps be expressed—not (as has hitherto always been tried) as a function of the co-ordinates (y, z) of the point, but rather as a function either—

1°, of the "minimum effective distance" of the point from the wet border.

2°, of the "average effective distance" of the point from the wet border,

and preferably of the latter.

As different parts of the wet border (*e.g.*, the air and the solid border) exert resistance of very different intensity, it seems necessary to include both the distance and the "specific resistance" in these expressions: thus the term "effective distance" in the above, means the actual distance (from any part of the wet border) multiplied by some co-efficient expressing the "specific resistance" (*of that part* of the wet border).

With the following notation :—

dB = length of any element of the wet border,

ρ = co-efficient of specific resistance thereof, (varying along the wet border),

r = distance thereof from the point y, z.

Then the—

$$\left.\begin{array}{l}\text{"Average effective distance" of the}\\ \text{point } (y, z) \text{ from the wet border}\end{array}\right\} = \int r \,.\, \rho dB \div \int \rho dB, \quad \text{..........(12)},$$

the integrals being carried right round the wet border.

This expression will always be one of such great complexity (even for the simplest form of cross-section) that to apply it in numerical calculation would be very laborious: and at any rate the attempt to seek the form of the function by numerical trials from direct Experiment—in the absence of any indications from a rational Theory—would be quite hopeless.

CHAPTER XIX.

AREAS AND DISCHARGES.

Preface.—This Chapter contains a detailed account of the mode of Area- and Discharge-Computation (Art. 1—10 & 14—15a) from the data of Ch. V, XVII, with discussion of the Errors involved (Art. 11, 13, 19—19d); and comparison with other processes 16—16c, 20—22a; also discussion of the dependence of the Discharge on the External Conditions (Art. 17—18, 20g). The most interesting Articles are—omitting details—Art. 1—3, 11—16a, 17—20, 20e—22a.

1. Areas and Discharges.—It is convenient to explain the computation of the three following quantities together, as to a great extent the same formulæ are used for each:—

1°. Cross-Section Area, (A).

2°. Superficial Discharge (D) past a Transversal, (measured by the area of the corresponding Velocity-Curve.)

3°. Cubic Discharge (**D**) through a cross-section, (measured by the volume of the Velocity-Surface).

The data for determining these are respectively—

1°, a system of depth-ordinates (H_y) in the Cross-Section.

2°, a system of velocity-ordinates (v_y, or u_y) in the Velocity-Curves.

3°, a system of areas ($D_y = H_y . u_y$) of the plane sections of the Velocity-Surface,

with the same abscissæ ($\pm y$), *i.e.*, at the same points of the Transversal.

[The spacing of this system of corresponding ordinates (H_y and v_y or u_y) and areas (D_y) has been fully explained in Ch. XVII, 5—6f, and Abstr. Tab. 11, (which also shows the formulæ actually used at each Site)].

Hence the three quantities—the Area (A), Superficial Discharge (D) past a Transversal, and Cubic Discharge (**D**) may be *calculated by approximation-formulæ of the same* type*, (a great convenience in practical calculation.)

[By far the most important part of this Chapter—in a practical sense—is that which relates to the CUBIC DISCHARGE.]

2. Notation.—The following Notation will be used:—

Let y be the distance of any Float-course from the centre.

" H_y " Average Depth along the Float-course whose abscissa is y.

" v_y " Float-velocity (at any level) along that Float-course.

* Moore's Elementary Mensuration, Art. 114, 122.

Let u_y be the Rod-velocity along that Float-course.

„ D_y „ (superficial) Discharge past the vertical whose abscissa is y.

„ D „ (superficial) Discharge past any Transversal.

„ b „ breadth of cross-section on that Transversal.

„ U „ Mean Velocity past that Transversal.

„ **D** „ (cubic) Discharge through the whole section.

„ **V** „ Mean (sectional) Velocity.

For distinction also the symbols H, v, u, D, will receive the subscripts e, t, q, m to denote the particular values at the edge (e), over top immersed step (t), and at the quarter-point (q), and middle-point (m) of the Side-space respectively.

3. Approximation-Formulæ.—It seems convenient to quote here the several approximation-formulæ for AREAS and VOLUMES in concise form, so far as required for these Experiments.

Let X_y be the type-symbol of a number of quantities *of same kind*, spaced at equal distances (β) along a base-line.

And X_y'', X_y' the type-symbols of those quantities (like X) which are at equal distances ($\pm y$) to left or right of the centre ($y = 0$) of the base-line.

Here X_y are either ORDINATES (as H_y, v_y, u_y) of the curve whose Area is required, or AREAS of parallel plane sections (as D_y) of a surface whose Volume is required.

Then the formulæ are as follows:—

i. *Simson's Rule.* (Quantities of type X at equal spacing β).

$$\left.\begin{array}{l}\text{Area or}\\ \text{Volume}\end{array}\right\} = \tfrac{1}{3}\beta\,.\,(X_1''+4X_0+X_1'),\ \text{between } X_1'' \text{ and } X_1', \ldots\ldots\ldots\ldots(1a),$$

$$= \tfrac{1}{3}\beta\,.\,\{(X_2''+X_2')+4(X_1''+X_1')+2X_0\},\ \text{between } X_2'' \text{ and } X_2',\ (1b),$$

and so on by repeated application.

[This Rule, when used for finding volumes, is also termed the "Prismoidal Formula"].

ii. *Cubic Rule.* (Quantities X_3'', X_1'', X_1', X_3', at equal spacing β).

$$\text{Area or Volume} = \tfrac{3}{8}\beta\,.\,\{(X_3''+X_3')+3\,(X_1''+X_1')\}, \ldots\ldots\ldots\ldots(2).$$

iii. *Weddle's Rule.* (Quantities X_3'', X_2'', X_1'', X_0, X_1', X_2', X_3'), at equal spacing (β).

$$\text{Area or Volume} = \tfrac{3}{10}\beta\,.\,\{(X_3''+X_1''+X_0+X_1'+X_3')+5\,(X_2''+X_0+X_2'\}, \ldots(3).$$

These Rules are severally applicable to Spaces which are divided into 2, 3, or 6 equal Sub-spaces respectively, and also (by a repeated application) to Spaces divided into n equal Sub-spaces, where n is a multiple of 2, 3, or 6.

[They are equivalent to the following* suppositions:—

Areas. The ordinate (X_y) is assumed to be a quadratic, cubic, or sextic function of the abscissa (y).

Volumes. The area (X_y) of each parallel plane section is assumed to be a quadratic, cubic, or sextic function of the abscissa (y).

* Moore's Elemy. Mensuration, Art. 117, 118, 122; & Boole's Finite Differences, Ch. III, Art. 7.

[These formulæ are severally *accurate** when the function in question is a quadratic, cubic, or quintic respectively: and Weddle's* is highly approximate even when the function is a sextic, or septimic.]

iv. *Simson's Rule modified.* The case of a velocity-measurement missing at *one* of the quadrisections (q) of a quadrisected Space was of such frequent occurrence, that it was worth while forming a special Rule for it. The Rule applicable to a quadrisected Space is Simson's (No. i above): the missing quantity (say q) must be supplied by interpolation for use in the formula; it was always taken as the arithmetic mean of the two adjacent like quantities (say M and E). The Result expressed in terms of the known data (say e, Q, M, q, E) is of such simple form as to be worth quoting here for reference in like cases.

Let e, Q, M, q, E (Pl. XX, 8) be quantities of same type, (*e.g.*, all Velocities, or all Discharges past a vertical) at equal spacing β, whereof—

q not having been observed is *assumed* equal to $\frac{1}{2}$ (M+E),........................(4).

Then by Simson's Rule—

$$\text{Area or Volume} = \tfrac{1}{3}\beta.\left\{(E+e)+4(Q+q)+2M\right\},$$
$$= \tfrac{1}{3}\beta.\left\{(E+e)+4\left[Q+\tfrac{1}{2}(M+E)\right]+2M\right\}$$
$$= \tfrac{1}{3}\beta.\left\{(3E+e)+4(Q+M)\right\}, \qquad \text{.....................(5).}$$

which is the final Result required.

4. ACCURATE CROSS-SECTION AREA.—The Cross-Section Area (A) admits of *accurate* computation (1°) as a whole when its whole contour is either regular or permanent; or 2°, in part, wherever its contour is either regular or permanent. In such cases of course the Area was accurately computed, the use of the approximation-formulæ being unnecessary. These cases occurred as follows:—

1°. *Permanent contours.* In the Solání Twin Aqueducts, (Pl. II, 4): in these (being in masonry channels) the *whole* Area was accurately computed, (*see* Tab. V.)

2°. *Banks regular or permanent.* The cases were—

(a), Regular earth side-slopes, at the 15th Mile, New Site, (Pl. II, 1.)

(b), Masonry steps, at the Solání Embankment Sites, (Pl. II, 2.)

(c), Masonry side-slopes, at the Belra and Jaolí Sites, (Pl. IV, V.)

In these three cases, the Side-Space Areas (over the regular or permanent banks) were accurately computed.

[In what follows then, the use of the approximation-formulæ for Cross-Section Area-computation applies *only to irregular beds*].

5. CUBIC DISCHARGE, *Preliminary Step.*—It will be seen that the quantities (X_y) of the formulæ are—in the case of Cubic Discharge computation—the superficial Discharges (D_y) past the several verticals whose abscissæ are y, so that (except when the bed is level), these quantities must be prepared by a preliminary computation, by the formula,—

$$D_y = H_y \,.\, u_y, \qquad \text{.....................................(6),}$$

i.e., by multiplying *separately* every Rod-velocity (u_y) by the Average Depth (H_y) along the Float-course.

[This preliminary work of preparing these products is the most laborious part of the work when the multiplication has to be done in the usual way. But it can be

* Moore's Elementary Mensuration, Art. 117, 122; Boole's Finite Differences, Ch. III, Art. 7.

done in a few minutes with the help of a Crelle's large Multiplication Table when neither factor contains more than 8 figures, (Ch. V, 26)].

5a. *Rectangular Section.*—In such a section with level bed and vertical sides the depth (H_y) is constant right across, and would therefore enter as a *constant factor* into all the superficial Discharges (D_y). The Cubic Discharge may therefore be more rapidly computed by calculating first the Discharge (say D_m) past the Mean Velocity Transversal,—measured by the Area of the Mean Velocity Curve,—and multiplying this Result by the constant depth (H), so that—

$$\mathbf{D} = D_m \cdot H, \quad \text{..................................} (7).$$

6. CENTRE- AND INTER-SPACES, (Abstr. Tab. 11).—Formulæ of the same type (*see* Tab. 11) were used then for each of the three quantities—

1°. Cross-Section Area (A), (when over an irregular bed),
2°. Discharge past a Transversal (D),
3°. Cubic Discharge (**D**),

in the Centre-Space, and also in the Inter-Spaces. The Table explains sufficiently.

7. SIDE-SPACES.—The formulæ used were varied (*see* Tab. 11) according to circumstances; the edge-velocity (v_e or u_e) being taken zero when entering into the computations (*see* Ch. XVII, 14d). The following cases require special explanation:—

(*a*). Side-Space not sub-divided. (*b*). Side-Space quadrisected.
(*c*). Side-Space over masonry steps.

7a. *Side-Space not sub-divided.*—This occurred as follows:—

1°. *Soláni Twin Aqueducts.* No velocity-measurements under the corbelling; and in a few cases none in the Side-space near the central Pier.

The Velocity-Curve being *known to be convex* (Ch. XVII, 12, (3*c*); & Pl. XX, 7, 9), its Side-Space Area (when not sub-divided) was computed by the formula—

$$\text{Area} = \tfrac{2}{3} \times \text{circumscribing rectangle} = \tfrac{2}{3} \times \beta \times v \text{ or } u, \quad \text{.......................} (8),$$

where β = width of Side-space, (usually assumed to be 2·5′, *see* Art. 9, 3°.)
v or u = velocity-measurement nearest the edge.

[This is equivalent to supposing the curve of parabolic shape in the Side-space].

2°. *Distributaries.* No soundings or velocity-measurements in the Side-spaces.

The Side-Space Areas were computed as Triangles, and the Side-Space Cubic Discharges as Wedges (Pl. XX, 10) by the formulæ—

$$\text{Side-space Area} = \tfrac{1}{2}\beta H, \quad \text{Cubic Discharge} = \tfrac{1}{2}\beta \cdot Hu, \quad \text{.......................} (9),$$

where β = width of Side-space.
H = Average Depth along Float-Course bordering the Side-space.
u = Rod-velocity along " " " " "

7b. *Side-Space quadrisected,* (Simson's Rule modified).—This case occurred (Tab. 11) in the Side-spaces at the following Sites, the velocity-measurement (v_q or u_q) at the quadrisection nearest the centre (marked in the Table with a dagger †) being wanting in each case:—

1°. 15th Mile, New Site, (no u_q between 80′ and *m*, *see* Pl. XXV).
2°. Soláni Embankment Main Site, at low water, (no $u_{71\frac{1}{4}}$, *see* Pl. XXXVI).
3°. Soláni Twin Aqueducts, near central pier, (no $v_{40\frac{5}{8}}$, or $u_{40\frac{5}{8}}$).

The edge-velocity (v_e or u_e) was assumed zero in each case, so that the Modified

Simson's Rule (Art. 3, iv) here assumes the simple form, (since $e = 0$ therein, *see* Pl. XX, 8, wherein the interpolated quantity q is marked by a dotted line,)

Area or Volume $= \frac{1}{3} \beta \cdot \{3E + 4(Q + M)\}$,.................................... (10).

7c. *Side-Space over steps.*—This case occurred at the Solání Embankment Main Site, at High water, in which case the Side-spaces comprised only the area over the immersed steps. The velocity-measurements were made at the points explained in Ch. XVII, 6f, 2°, and shown in Pl. XXV. The following special* formulæ were used for the (Cubic) Discharge-computation.

[No Surface-, Mid-depth-, or Bed-Velocity work was done at this Site].

N.B. See Plate XXV.	Steps immersed. Top.	Quarter.	Middle.	Lowest.	Steps immersed.	FORMULA FOR CUBIC DISCHARGE (𝐃). [D = H . u in each case].	Result.
					1	$\beta . D_{75\frac{1}{2}}$	(11*a*).
Reference-symbol, ..	t	q	m	$75\frac{1}{2}$	2	$\beta . (D_t + D_{75\frac{1}{2}})$	(11*b*).
					3	$\beta . (D_t + D_m + D_{75\frac{1}{2}})$	(11*c*).
Depth over step (H),	H_t	H_q	H_m	$H_{75\frac{1}{2}}$	4	$\beta . (D_t + D_q + D_m + D_{75\frac{1}{2}})$ ..	(11*d*).
					5	$\beta . (D_t + D_q + 2D_m + D_{75\frac{1}{2}})$ ..	(11*e*).
Rod-velocity (u), ..	u_t	u_q	u_m	$u_{75\frac{1}{2}}$	6	$\beta . (D_t + D_q + 2D_m + 2D_{75\frac{1}{2}})$ ..	(11*f*).
					7	$\beta . (D_t + 2D_q + 2D_m + 2D_{75\frac{1}{2}})$	(11*g*).
Superficial Discharge } (D=Hu),	D_t	D_q	D_m	$D_{75\frac{1}{2}}$	8	$\beta . (2D_t + D_q + 2D_m + 3D_{75\frac{1}{2}})$	(11*h*).
					9	$\beta . (2D_t + 2D_q + 2D_m + 3D_{75\frac{1}{2}})$	(11*i*).

It will be seen that the formulæ are based on the assumption that the Superficial Discharge past each of the verticals on which the Rod-velocities were measured is approximately the mean Superficial Discharge past any vertical throughout the width of either 1, 2, or 3 steps, according to the co-efficient used.

[This assumption of course leads to errors of excess and defect in several cases, but—on examining the formulæ—it will be found that the co-efficients are so arranged that the partial gains or losses over certain steps compensate each other to a great extent in each formula taken as a whole, so that the Resultant Error of excess and defect due to this assumption must be small].

8. Solani Twin Aqueducts.

—The procedure at these Sites was to a certain extent special:—

8a. Cross-Section Area (A).—This was *accurately* computed (Art. 4).

8b. Discharges (D and 𝐃).—The Cross-Section of each Aqueduct is so nearly rectangular that—viewing the uncertainty of such computation—it did not seem worth while in Discharge-computation to take account of the slight departures from the strict rectangular form, viz., (Pl. II, 4)—

* These were devised to meet the present case, to which none of the preceding ordinary formulæ (of Art. 3) are *properly* applicable in consequence of the two outer velocity-measurements u_t, $u_{75\frac{1}{2}}$ having been effected over the centres of the top and lowest immersed steps, and *not at the edges* of the Side-spaces as required by the ordinary formulæ. On further consideration, it appears, however, to the Author doubtful whether these new formulæ have any advantage in accuracy, and whether in fact it would not have been at least equally accurate to have used Simson's or Simson's modified Rules (accepting the values u_t, $u_{75\frac{1}{2}}$ as *sufficient approximations* to the two velocities u_e, u_{75} *at the edge* and *at the inner boundary* of the Side-space respectively); which use would have had the practical advantage of not increasing the number of formulæ in use.

1°, the contraction of the width (from 85′ to 82′) by the corbelling of the outer footways above the 7′·3 level.

2°, the slight contraction of the width above the bed (from 85′ to 83′½) below the 2′ level.

Accordingly all the Discharges past a Transversal, and all the Cubic Discharges *have been computed as if the width were* 85′ uniformly from surface to bed, *i.e., as if the side-walls were vertical throughout.*

This affects all the Surface-work (which was all above the 7′·3 level), the work near bed-level, and the Cubic Discharges, so that—as far as this source of error goes—

"The Surface-, Bed-, and Cubic Discharges are all over-estimated",..........(12).

But the actual error must be in all cases small, and *in many cases quite trifling*, thus—

1°. *Surface-Discharges.* These are the most affected, as the surface-contraction sometimes amounted to 3′·0 (when the water-surface was above the 9′·85 level, Pl. I, 3).

[The maximum error is probably $\frac{2}{3} \times 3{\cdot}61 \times 3{\cdot}00 = 7{\cdot}22$ *sq. ft. per sec.*, in Ser. 53, Set No. 13].

2°. *Mid-depth-Discharges.* Not affected.

3°. *Bed-Discharges.* Error quite trifling.

4°. *Cubic Discharges.* Error always very small (compared to the whole Discharge), and altogether trifling when the water-surface was below the 7′·3 level.

[The propriety of this procedure is no doubt* questionable, especially in the case of the Surface-Discharges. It *seemed to offer some advantage* even in this case at the time, in enabling them to be compared together more readily (being all computed as for a Transversal of same (85′) width). And in the case of the Cubic Discharges (although no doubt some reduction could have been made), the appropriate reduction is by no means obvious].

8c. CUBIC DISCHARGE, (D).—The neglecting (in computation) the departure from the true rectangular form, (involving the assumption of constant depth (H) right across,) enabled the Cubic Discharge to be computed by the comparatively simpler process applicable to rectangular sections, (Art. 5a.)

9. *Departure from exact spacing.*—In a few cases practical difficulties prevented the Float-courses being placed quite in the proper position required for use in the approximation-formulæ. In these cases the velocity-measurement was accepted as *a sufficient approximation* (for all purposes of Discharge-calculation) to the velocity in the proper position. The cases were (*see* Tab. 11)—

1°. *Fifteenth Mile, Old Site.* The velocity-measurements at 84′ from the centre were accepted as equivalents of the velocity at 85′, (where required for the Cubic Rule.)

2°. *Soláni Embankment, Main Site* (at High water). The velocity-measurements at 74½′ from the centre (*i. e.*, close to the 4′ drop-wall) were accepted as equivalents of the velocity at 75′ (as required for the Cubic Rule).

* The full data are given in Det. Tab. XXIX—XXXII, XXXIV—XLI, & LVII, so that the computation could be done anew by the reader if wished.

8°. *Soláni Twin Aqueducts.* The velocity-measurements at 39½′ from the centre (*i. e.*, close to the corbelling, were accepted as equivalents of the velocity at 40′, (as required for Simson's Rule); this leaves 2′·5 for width of the Side-space under the corbelling, (*see* Pl. II, 4.)

This approximate usage is believed to be sufficient for the purpose, and greatly simplifies the labor of computation.

10. *Missing Data.*—It happened occasionally that a velocity-measurement required for insertion in the approximation-formulæ was missing from some cause or other. This was usually supplied by simple proportional* interpolation between the two adjacent velocity-measurements.

[*Example. See* "Simson's Rule modified" (Art. 8, iv) to suit the case of a velocity-measurement missing at *one* of the quadrisections of a quadrisected Space. This was the only case of frequent occurrence].

Occasionally, however, the interpolation was avoided by applying other formulæ (than the usual one noted in Tab. 11) when suitable in the changed circumstances.

Examples. These cases occurred chiefly at the Soláni Aqueduct—

Ex. 1. *Side-space near central pier.* Sometimes the velocity-measurement at 41¼′, *i. e.*, next to the pier, was wanting. In this case Simson's Rule was applied, the data being at 40′, 41¼′, and 42½′ (the last velocity assumed zero).

Ex. 2. *Side-space near corbelling.* Sometimes the velocity-measurement at 39½′ *i. e.*, next to the corbelling was missing. In this case the Cubic Rule was applied to the Inter-space, the data being at 30′, 32½′, 35′, 37½′; and the Parabolic Rule to the Side-Space, (which extended from 37½′ to the assumed edge at 42½′.)

11. Formulæ-Errors.—The effect (upon the Result) of an error in any of the observation-data is proportional to the weight of that quantity in the formulæ in which it is to be used, and therefore to its co-efficient in those formulæ. Thus certain data are *more important than others*, (carrying larger co-efficients in the formulæ), and are *of different importance in different formulæ*, (the co-efficients differing): thus—

1°. *Simson's Rule.* The even quantities are four times, and the odd ones twice as important as the end quantities.

2°. *Cubic Rule.* The middle quantities are thrice as important as the end ones.

3°. *Weddle's Rule.* The even quantities are five times, and the middle one is six times as important as the end quantities.

Hence the effect (upon the Result) of an error in any of the data is immensely greater in the case of the more important quantities (in proportion to their weight). It seems impossible to provide adequately for this difficulty except by greater care in the observation of these more important quantities, and the only obvious plan is to reduce all the partial results to equal weight by providing that the risk of error in the observa-

* The *best* mode of supplying a missing observation would undoubtedly have been that given in Works on the Calculus of Finite Differences (*see* Boole's Work, Chap. III), but the labor of such interpolation would have been quite prohibitory.

tion-data shall be inversely proportional to their weight in the computation-formulæ. This requires that—

"The number of repetitions of any observation-measurement should be proportional to its weight in the computation-formulæ",................................(13).

[This is only given as a suggestion towards future improvement of such observations: its advisability was not perceived till a late period of these Experiments].

12. *Above formulæ simple in use.*—It might be supposed that the formulæ above given—judging merely from the number of pages taken to describe their use—were troublesome of application, but this is not the case: the co-efficients involved are all so simple, and the order in which the odd and even velocity-ordinates succeed each other is so regular and simple, that with a little method* in the way of using them, they are *extremely simple and easy* of application, and can be quickly picked up by the most ordinary computer, at the same time that there is *considerable gain in accuracy* in their application. The use of ruled forms especially facilitates methodical work.

[A specimen page of computation of a Cubic Discharge by these formulæ is given in Abstr. Tab. 34, preceded by the Field-Book (Tab. 33) containing the Field-data. The details given are those of 9-1-'79 in Ser. 201].

13. **Simpler Approximations,** *Err in defect.*—Simpler approximation-formulæ than those given above will generally err *constantly in defect* in the long run, (though not necessarily in any single instance.)

The two following classes of Results:—

1°. (*a*), Cross-Section Area (A),

(*b*), Superficial Discharge past a Transversal (D),

(*c*), Cubic Discharge (**D**) in a rectangular channel,

2°. Cubic Discharge (**D**) in a concave bed,

are affected to a very different extent.

The former—all under Class 1°—being all measured by the Area contained within their respective bounding curves, the only simpler formulæ that could well be proposed would be the Trapezoidal and Arithmetic Mean Rules quoted in Ch. XIII, 2d. And since these Curves are generally concave towards their several Base-Lines, it follows (precisely as in that Article) that both these Rules must *err in defect* in the long run.

[The Error is of course not very large; still there being sufficiently simple and more accurate Rules available, there can be no excuse for not using them].

But, when applied to the Cubic Discharge over a concave bed, there is considerable risk of introducing an error in defect *in two ways;* viz., both—

1°, through the general concavity of the bed,

2°, through the general down-stream convexity of the Mean Velocity-Curve,

either of which separately would produce its own small error (constantly of defect). But when these two independent sources of error are com-

bined—as they certainly would be by certain ways of application of the simpler approximations (such as the Trapezoidal and Arithmetic Mean Rules)—the error resulting is *no longer a small one* (unless indeed the variations of both velocity and depth throughout the greater part of the cross-section be themselves very small), and is *constantly one of defect.*

[This could not be easily proved in any very short compass, and must be accepted as a known mathematical Result].

14. Present Results.—The formulæ above described (Art. 3—8) were applied as follows:—

Area-Computation. Only certain "fundamental Areas" were computed direct from the formulæ, and the rest were interpolated. This is fully explained in Ch. V, 18—19b.

Discharge-Computation. No interpolation was here admissible. The Discharge-Measurements (D or **D**) were computed direct from the formulæ *separately for every SET* of velocity-work past a Transversal, and are shown in Col. 7 throughout Tab. XXIX—LVII, and Col. 8 throughout Tab. LVIII—LXX. These are only FAIR DISCHARGE-MEASUREMENTS according to the principle of Ch. VI, 16. The Means at the foot of the several Series are the best AVERAGE values that the ever-varying state of the Canal permitted to be obtained, nearly freed from Observers' personal equation (Ch. VI, 13).

An Abstract of the Results (Mean and Range of each Series) is given in Abstr. Tab. 13—18 & 20—22.

15. Cubic Discharge, RECAPITULATION.—The measurement of the Cubic Discharge passing through a channel is so important a matter in practical Hydraulics, that it may be well to recapitulate briefly the process used in these Experiments. The process naturally divides itself into three very distinct Steps.

STEP I. Obtaining the AVERAGE DEPTHS along a number of lines (Sounding-Courses) parallel to the current-axis, by sounding over a number of cross-sections.

STEP II. Measurement of the MEAN VELOCITIES past the verticals through the points where those lines meet the central cross-section.

STEP III. Computation.

The time required for each Step (at any particular Site) depends of course entirely on the approximation arrived at, *i.e.*—

1°, on the number of Cross-Sections sounded over.

2°, on the number of verticals of Mean Velocity-measurement.

3°, on the number of repetitions of each velocity-measurement.

In the present Experiments the time occupied was as follows at the large Sites:—

STEP I. *Sounding* along 15 to 17 Sounding-Courses and in 8 Cross-Sections. Time 3 to 4 hours.

STEP II. *Mean Velocity-measurements* in 15 to 21* Float-Courses; each thrice repeated. Time 2 to 4 hours.

STEP III. *Computation.* About 2 hours.

[This last Step (Computation) includes the following :—

1°, Reduction, Inking, and Checking Field-Book, *see* Tab. 33.

2°, Discharge-computation, *see* Tab. 34, original and checking.

A number of such computations can, however, be done proportionately more rapidly than a single one].

15a. ADVANTAGES OF PROCESS.—The chief advantages of this process are the two following :—

1°. Velocity-measurements are obtained from many different parts of the channel; so that the final Result is certainly some sort of Average Result of the whole, independently of all Theories.

2°. The whole Field-work of the velocity-measurement is carried out within a moderate time (say 2 to 4 hours at large Sites), within which the External Conditions depending on the Control—excluding therefore the Wind—would be generally tolerably constant.

This last advantage *cannot be overrated:* it renders unnecessary the application of any so called "correction" to reduce the Results computed for one part of the channel to comparability with those computed for another under changed External Conditions (*e. g.*, changed water-level, or changed surface-slope), or to reduce actual velocity-measurements to mean velocities. The necessity of applying any such "corrections" is a great disadvantage, as in the present state of Hydraulic Science they must be looked on as to a great extent hypothetical.

The comparative speed with which the whole Field-work can be carried through depends of course on the power of *rapidly obtaining the sought velocity-data* (Mean Velocity past each vertical) *by some single operation*, and depends therefore in the present Experiments on the use of the Mean Velocity-Rod.

This involves of course the fundamental hypothesis (proved true in Ch. XV, XVI) as to the sufficient approximation of the Rod-velocity to the sought Mean Velocity past a vertical, but this is the *only hypothesis made.*

16. **Cubic Discharge,** OTHER EXPERIMENTS.—It will be instructive to compare the mode of computing the Cubic Discharge employed in the other large Experiments.

16a. CHANGED CONDITIONS, *Other Experiments.*—In the Experiments on the large Rivers, (Mississippi, Lake River, Connecticut, Irrawaddi,) it is by no means clear

* The number of Float-Courses sometimes exceeded the number of Sounding-Courses; viz., at Sites with Regular Banks, the Cross-section of which was obtained once for all (Ch. V, 12a) by measurement.

(from the published Reports) whether the whole of the velocity-measurements from which each separate Discharge was computed were executed in one and the same day. But it seems almost certain—partly from internal evidence* in the Text of those Experiments, and partly from the great difficulty of the field-work on these large rivers—that *in very many of the cases*† the complete Set of data were not collected in one and the same day, but in part on many different days.

The combination* of data must in this case have been made chiefly from data *at nearly the same water-level*, (with a selection perhaps of cases in which the velocities were somewhat similar.) But the evidence of the present Experiments of *great variations* in Discharge with nearly same water-level, (even with nearly same Surface-Falls in the Sub-Reaches above and below the Site,) is sufficient to show that—in the present state of hydraulic science—the approximation to similarity of External Conditions is very uncertain, so that such combinations should be avoided if possible. To enable this to be done, it is essential that the *whole of the data* for any one Discharge-measurement *should be collected within a few hours.*

16b. *Miss. Report*, (pp. 226—228, 262, 263).—The total breadth (b) of the Cross-section was divided into Spaces of 200′ width. The velocity deduced from the mean of the timings of all Floats passing within any 200′-Space was accepted as the *approximate mean velocity* (say $\mathbf{u}$) through the whole area (say $\mathbf{a}$) of that Space, (ascertained by sounding), so that,

Approximate (cubic) Discharge through any 200′-Space $= \mathbf{ua}$.

$$\left.\begin{array}{r}\text{Approximate Total}\\ \text{Cubic Discharge } \mathbf{D}\end{array}\right\} = \Sigma\,(\mathbf{ua}), \left\{\begin{array}{l}\text{the sum of all such partial Discharges}\\ \text{throughout the whole breadth of the river.}\end{array}\right.$$

The velocity-measurements were distributed in two ways, of very different value—

1°. At Carrollton in 1851,—At all depths.

2°. In 1857-58,—Only on a Transversal at 5′ depth.

In the former case, the Result $\mathbf{D}$ was considered *final*. In the latter case it was considered only as approximate, and was "corrected" by multiplying by a certain co-efficient of correction, viz., by the ratio $U_m \div U_s$, where—

U_m = grand mean of the mean velocities past all verticals,

U_s = mean velocity past the 5′-depth transversal,

deduced from their vertical velocity-curves.

With the *data available*, it seems difficult to suggest any better process, (the data being insufficient especially in Case 2°,) though it is easy to see some points wherein an improvement is desirable, viz.—

1°. The use of the arithmetic mean of the velocities in any one 200′-division as the Mean Velocity through the area corresponding gives a result somewhat too small (when the velocity-surface is wholly convex down-stream), even when the bed is level; but if the bed be concave, and the partial areas be also under-estimated—say through use of the less accurate formulæ—the Resultant Error (always in defect) will be probably no longer a very small one (Art. 13).

2°. The mode of determining the value of the ratio $U_m \div U_s$ is also open to great doubt. It appears (pp. 260, 263, *ib.*) equivalent to assuming that the *same* parabolic

* The passages are too numerous to quote.

† In fact 150 Floats seem to be about as many as can be observed in one day on a large river, *see* Lake Survey Report of 1869, p. 566.

relation exists between the *Mean Velocities* past each transversal from surface to bed as between the velocities at different points of any one vertical from surface to bed.

16c. *Lake River Report of* 1869, (pp. 571, 575).—Velocities were observed at every 5′ of depth on many verticals usually at 200′ apart, except near the banks, where special spacings were used : this of course occupied many days. Again, velocity-measurements were made at many points—at same (200′, &c.) spacing as before—on one or more transversals (at selected depths), the Set on any one transversal being carried out *within a single day*. By help of these last the velocities past the several verticals were reduced to what they would have been if all observed on the same day, and also to the centres of the 200′ spaces.

[The mode of reduction is not explained, but *see* Art. 16a (above) as to the extreme uncertainty of such reductions].

Each "reduced velocity," (say $\mathfrak{u}$) was multiplied by the partial area $\mathfrak{a}$ to which it applied, and the Total Discharge $\mathfrak{D}$ was found as the sum $\Sigma(\mathfrak{u}\,.\,\mathfrak{a})$ of all such products.

16d. *Connecticut Report of* '78 (pp. 352, 353).—The mean velocity (U) past each vertical was multiplied by the Average depth (H) in the Float-Course: and the sum of these products Σ (UH) was divided by the sum of the depths Σ (H). This was accepted as the equivalent of the Mean (Sectional) Velocity ($\mathbf{V}$); so that the Cubic Discharge was found by multiplying this value of $\mathbf{V}$ by the Cross-Section Area, so that $\mathbf{D} = \{\Sigma\,(\mathrm{UH}) \div \Sigma\,(\mathrm{H})\} \times \mathrm{A}$. The Result is admitted to be "slightly too small", (p. 353.)

In some cases the Discharge was computed from Mid-depth-velocity measurements: in this case a sort of Mid-depth Mean Velocity (say ($\mathrm{U}_{\frac{1}{2}\mathrm{H}}$) was computed in same way as above, viz., $\mathrm{U}_{\frac{1}{2}\mathrm{H}} = \Sigma\,(v_{\frac{1}{2}\mathrm{H}}\,.\,\mathrm{H}) \div \Sigma\,(\mathrm{H})$. This was multiplied by the ratio ($\mathrm{U} \div v_{\frac{1}{2}\mathrm{H}}$) of the Mean Velocity past a vertical (U) to the Mid-depth velocity on that vertical ($v_{\frac{1}{2}\mathrm{H}}$) (as ascertained by Experiment on a certain number of verticals at the Section), and the Result was accepted as the equivalent of the Mean (Sectional) Velocity, ($\mathbf{V}$); and the Cubic Discharge ($\mathbf{D}$) was then found as the product AV.

[This is of course open to the objection that it involves the assumption that the ratio $\mathrm{U} \div v_{\frac{1}{2}\mathrm{H}}$ is *constant for all verticals*, and equal to the *required* ratio $\mathbf{V} \div \mathrm{U}_{\frac{1}{2}\mathrm{H}}$].

16e. *Irrawaddi Report of* '79 (p. 21).—The whole width (b) of the river was divided into a certain number of Spaces (of unequal width) within each of which the surface-velocity was found to be nearly constant. The mean of all surface velocity-measurements within each space was taken as the Average Surface-velocity (v_0) of that Space.

This was multiplied by the ratio ($\mathrm{U} \div v_0$) of the Mean Velocity (U) past verticals within that space to the surface-velocity (v_0) (ascertained by Experiment within that space, and at nearly the same water-level), and by the partial cross-sectional area ($\mathfrak{a}$) within that space. These Results are the partial Discharges through the several Spaces; and the Total Discharge was found as the sum of all such.

[In the application of this process, the approximation of course depends on the legitimacy of the ratios $\mathrm{U} \div v_0$ used. The use of a special value of this ratio for each space, and the change thereof with change of water-level is (theoretically) a great improvement on the process as used in the Mississippi and Connecticut computations].

17. **Discharge-Variation.**—From the mode of computation, it is obvious that the Discharge-measurements obtained depend on two to three factors, as follows :—

1°, *Discharge past a Transversal* (D), on two factors, viz., breadth and velocity.
2°, *Cubic Discharge* (**D**), on three factors, viz., breadth, depth, and velocity.

Of these, the depth (which affects only the latter) and the velocity are alone subject to great variation. The breadth either remains constant (as in rectangular sections), or else changes with the depth, the utmost change being, however, only a small fraction of the whole breadth : this change is usually one of increase with increase of depth ; the reverse change of decrease with increase of depth occurring only in exceptional cases (as in the Soláni Aqueducts under the corbelling).

The change of Discharge (of each kind) due to change of breadth—being small—is liable to be quite masked in the changes due to change in the other factors, and is not worth discussing.

Discharge past a Transversal.—This Discharge will thus obviously increase and decrease along with the remaining factor (velocity) : this requires no discussion.

17a. Cubic Discharge.—The two chief variable factors (depth and velocity) in the Cubic Discharge-measurement are liable to great and independent variation, so that it might be expected to increase and decrease with them *jointly*—not necessarily with increase or decrease of one only.

A glance down Col. 7 of Abstr. Tab. 14—18, wherein the Series are arranged *by falling water-level* will show at once that *as a rule*—

" The Cubic Discharge at any Site increases and decreases rapidly with rise and fall of water-level ",..(14).

This Result may be *confirmed* by examining the detailed (Discharges in Col. 7 of Det. Tab. XXXIV—LVI), which are also arranged within each Series generally by falling water-level ; *a general decrease* (with, however, many irregularities) *will be obvious on the whole.*

[Ser. 106, 112, 151, 156, 165, 196, 216, 217, 222, 224 afford pretty good instances of this].

[As to the irregular decrease within a Series, it is to be remarked that the fall of water-level admitted within a Series is so small, that any effects due to it are liable to be masked by even small changes in the other factor (velocity). The Unsteady Motion alone is sufficient to cause irregularities, the individual Discharges depending on only a limited number of velocity-measurements].

The effect of velocity on the Result is well seen in the following Abstract (arranged by falling water-level for each Site), which contains only the Average Results of the Series quoted, so that the effect of Unsteady Motion must be in many of the cases fairly eliminated.

Solàni Twin Aqueducts.					Solàni Right Aqueduct [Left Aqueduct closed].				Solàni Embankment Main Site. High Water.				Solàni Embankment Main Site. Low Water.			
Aqueduct.	Series.	Sets.	Gauge-Reading.	CUBIC DISCHARGE.	Series.	Sets.	Gauge-Reading.	CUBIC DISCHARGE.	Series.	Sets.	Gauge-Reading.	CUBIC DISCHARGE.	Series.	Sets.	Gauge-Reading.	CUBIC DISCHARGE.
Left	102	12	9·63	2,922	131	2	4·60	481·9	157	7	8·12	4,832	170	2	3·64	1,124
	103	4	9·42	3,093	132	2	3·96	1,623	158	2	7·90	4,860	171	2	3·62	643·2
	—	—	—	—	133	1	3·60	212·0	—	—	—	—	172	2	3·58	483·3
Right.	120	9	5·78	1,584	134	1	3·58	979·3	161	11	6·82	3,868	173	5	3·47	820·5
	121	2	5·61	1,633	135	1	3·18	865·6	162	5	6·38	3,973	174	1	3·04	887·6
	—	—	—	—	136	1	3·12	740·2	—	—	—	—	175	5	2·90	1,142
	123	1	3·65	218·9	137	1	3·13	667·8					176	2	2·83	839·9
	124	1	3·49	722·2	138	1	2·88	620·7					177	4	2·42	852·9
	125	1	2·02	276·9	139	2	2·66	496·3					178	2	2·43	457·7
													179	4	1·92	626·0

These Results show such large, in several cases very large, departure from the general rule of increase and decrease with rise and fall of water-level, as to prove the other factor (viz., velocity) to be *of at least equal importance.*

17b. Velocity-variation.—In endeavoring to trace the dependence of the Discharges of each kind (D or **D**) on the External Conditions, it would be better then to eliminate the effects of variation of breadth and of water-level if possible. This can be done in a general way, viz., so far as the breadth and depth enter *directly into the Result* (as factors) by simple division of the Discharge (D or **D**) by the breadth (b) of the Transversal, or by the Cross-Section Area (A) respectively, according as the Discharge past a Transversal (D) or the Cubic Discharge (**D**) are under discussion. It will suffice then to discuss the dependence of the quotient (Mean Velocity of either kind, viz., $U = D \div b$, or $\mathbf{V} = \mathbf{D} \div A$) upon the External Conditions. This will be reserved for Ch. XX.

18. Cubic Discharge constant.—It seems almost certain *à priori* that even though the motion of particles be very unsteady, yet the Cubic Discharge is sensibly constant when the average free-level is steady for a length of time at several Gauges at a good distance apart, or shortly that—disregarding the small momentary oscillations of free-level—

"The Cubic Discharge is sensibly constant from instant to instant",.........(15).

In this way the Cubic Discharge differs markedly from the Superficial Discharges past a vertical or transversal, which are almost certainly vari-

able (*see* Ch. XIV, 4; & XX, 3a) from instant to instant. It follows that the term AVERAGE should not be applied to the Cubic Discharge in the same sense as to the Superficial Discharges.

[The causes of the considerable variations of the Cubic Discharge-measurements obvious in the Tables will be discussed later].

19. **Discharge-Error.**—An Error in estimating any of the factors (breadth, depth, or velocity, *see* Art. 17) which enter into the computation, will of course produce a corresponding error or uncertainty in the Discharge-measurements.

19a. BREADTH-ERROR.—An Error (Δb) in estimating the breadth (b) is—in a channel of great width compared to its general depth—of comparatively small importance *even in the absolute Results*, as from the mode of spacing adopted (chiefly at fixed points), the effect of an error in breadth would fall wholly in the "Side-spaces", in which both the velocities and depths are usually least: the greatest error possible would be—

"Max. Error in Discharge past a Transversal $= \mathrm{u} \cdot \Delta b$", ...(16a),

" „ „ Cubic Discharge $= \mathrm{H} \cdot \mathrm{V} \cdot \Delta b$",........................(16b),

where H = Mean Depth of Side-space,

„ u = Mean velocity past Transversal in Side-space,

„ v = Mean sectional velocity through Area of Side-space.

And, in comparing different Results at the same Site at nearly the same water-level, this Error is of trifling importance, the values of H, and—except for changed control—of u and of v also being then nearly alike. And further, in such comparisons even a considerable *constant* Error in breadth would matter little, as it would affect the Results nearly alike.

[Thus the over-estimating the Surface-breadth at the Soláni Aqueducts (Art. 8b) does not sensibly affect comparisons of Results *within the same Series*, the breadth-error being nearly constant throughout a Series].

19b. DEPTH-ERROR.—This affects only the Cubic Discharge-measurement. An error in depth (Δh) is of much greater importance in a channel of small depth compared to its width, than an error in breadth. If it arises from an error in sounding, it will (from the mode of computation) only affect the partial Discharge through the two Sub-Spaces adjoining the erroneous Sounding; but if it arises from an error in determining the water-level, it will affect every depth-factor alike, and in this case the Result is the comparatively large quantity,—

"Error in Discharge $= b \cdot \mathrm{V} \cdot \Delta h$",..(17).

As explained in Ch. V, 15, the depth-factors in these Experiments all depend ultimately on determinations of water-level, so that any cause producing error or uncertainty in water-level determination is liable to produce the above Error in the Result. This affects not only absolute Results, but also comparisons of Discharge-measurements at the same Site (even if done in succession,) as the depth-factors in each depend on different determinations of water-level.

19c. WIND-EFFECT.—From the above it is clear that a high wind may—independently of any real effect on the "forward velocities"—affect the Cubic Discharge-measurements merely through its disturbance of the water-level in the neighbourhood of the Gauge (Ch. V, 11, 15), so that—

"A cross wind is liable to cause excess or defect in Cubic Discharge-measurement according as it blows towards or from the Gauge",........................... (18).

It is worth noting that this Error could only be discovered by comparison of Discharge-measurements *at different Sites* differently exposed to Wind. It would be quite undiscoverable in comparing Discharge-measurements at the same Site, because the Wind affects the Gauge-Reading and Discharge-measurement *alike*, so that the Results would *apparently agree*.

[The maximum Error produced is probably very small (compared to the whole quantity), thus the greatest observed difference of level between the water-levels of opposite banks was only ·07′ (Tab. LXXVI), or probably ·035′ (or say ·04) of Error. This in a 200′ width with 5′·00 per second mean velocity would cause an Error of 40 *c. ft. per second*].

19d. VELOCITY-ERROR.—It has been already explained (Ch. VI, 16) that the single Discharge-measurements of all kinds in this Work cannot—in consequence of their depending on velocity-measurements, which are neither simultaneous nor yet proper average values—pretend to be *good* measures, but should be looked on only as FAIR DISCHARGE-MEASUREMENTS.

[It will be understood (Art. 18) that it is *not the Cubic Discharge itself that is subject to momentary variation;* but that the measure thereof, *i. e.*, the Discharge-measurement is an imperfect one in consequence of being derived from imperfect Averages].

20. Old and New Results (Abstr. Tab. 23).—The Table shows the Cubic Discharge of several of the Reaches corresponding to each half-foot of the Standard Gauge of the Reach, viz.—

(1), Roorkee Reach; (2), Belra Reach; (3) Kamhera Reach;

also the Cubic Discharge of the four small Distributaries for a few odd depths on their respective Gauges, both according to the Results of these Experiments, and according to the official Canal Tables in use at the time.

[The figures quoted for the present Experiments are interpolated roughly between the actual Results at the gauge-readings near each half foot: where very discrepant Results are obtained, the highest and lowest are given. There was unfortunately no official Table extant for the Jaolí Reach, so that no comparison is possible for that Reach].

20a. Canal Tables.—Before proceeding further, it is necessary to explain how the Canal Tables were prepared:—

20b. SOLANI AQUEDUCT AND BELRA TABLES.—These Tables were based on actual Experiment, *i. e.*, on a few *actual* Discharge-measurements (say D_1, D_2, &c., computed as explained below) at certain gauge-readings, (say h_1, h_2, &c.) These are the fundamental Discharges of the Table. Each of these (D_1, D_2, &c.) being divided by the corresponding Cross-sectional Area (say A_1, A_2, &c.) gives the corresponding Mean Velocity (V_1, V_2, &c.) The Mean Velocities (V) for all other gauge-readings (h) were interpolated by assuming that $V \propto \sqrt{R}$ throughout each interval, and lastly, the Cubic Discharges (D) were obtained as the products AV.

For the fundamental Discharges (D_1, D_2, &c.), surface velocity-measurements were

made at the middle points of each of three equal primary Spaces trisecting the surface-breadth. The Wet Borders (B'', B_0, B'), Areas (A'', A_0, A'), and Hydraulic Mean Depths (R'', R_0, R'), and also Bazin's co-efficients (c'', c_0, c') connecting Mean (sectional) Velocity with maximum velocity were computed separately for each Space. The Mean (sectional) Velocities (V'', V_0, V') of each Space were found by reducing the Average surface velocities (v'', v_0, v') by the corresponding co-efficient. The Cubic Discharges (D'', D_0, D') in each Space were found as the products $A''V''$, A_0V_0, $A'V'$; and the Total Cubic Discharge as the sum thereof.

[Note :—The values of R—the computation of which is the most laborious part of the process—are required solely for determining the co-efficients (c) which depend on R. The Wet Borders (B'', B') of the outer Spaces include both the bed and bank thereof: the Wet Border (B_0) of the Centre Space includes only the bed thereof].

The "fundamental Discharge-measurements" were mostly taken at the greater depths; from this it results that the Tabular Results at shallow depths practically hang on those at the greater depths being connected by the Rule $V \propto \sqrt{R}$ with *scarcely any check* available from actual Discharge-measurement in shallow water.

20c. Kamhera Bridge Table.—The Mean Velocities for each gauge-reading were computed from Bazin's Formula $V = C \cdot \sqrt{RS}$, with an *assumed constant value* $\frac{1}{3000}$ for S, (which is somewhat more than that of the bed). The Discharges were computed from the formula $D = AV$.

[It will be seen that this Table does not depend* on any actual velocity-measurements at all, but solely on a single ideal slope].

20d. Distributaries Tables.—These Tables were formed by interpolation (by precisely the same Method as for the Soláni Aqueduct Table) between a few fundamental Discharges.

The fundamental Discharges were computed from central surface velocity-measurements by the formula $D = c \cdot v_0 \cdot A$, where c is Bazin's co-efficient for reducing central surface velocity (v_0) to mean velocity (V).

20e. *Official Results small.*—There is strong reason to expect that the (fundamental) Discharges computed as above described would *usually err in defect*, as several causes—chiefly depending on the use (or misuse) of Bazin's co-efficient—combine together to produce defect.

1°. *Use of Bazin's Co-efficient.* This co-efficient was derived from small scale Experiments, (Ch. XX, 22,) in which the resistance of the bed and banks is much more influential than on the large scale: the value of the reduction-co-efficient so obtained is therefore probably too small for the large scale.

2°. *Use of Surface-velocities.* Bazin's co-efficient was certainly intended to be the ratio of Mean to *Maximum* Velocity ($V \div V$). The use of surface-velocities instead of maximum velocity causes an error in defect (Ch. XX, 22a).

3°. *Trisection of width.* Bazin's co-efficient is *not applicable to portions of a cross-section* of a channel, but *only to the cross-section as a whole.* The application to portions of a cross-section separately (as if they were independent channels) causes

* It is right to say that the Kambera Reach channel had been considerably widened about a year before the present Experiments were made, and that the Table in question was only a temporary one, awaiting the formation (for the altered conditions) of a Table based on actual Discharge-measurement similar to those of the other Sites.

error in defect increasing the more the additional observed velocities decrease (inasmuch as the (single) maximum velocity of the whole cross-section should have been used).

4°. *Area-Formulæ.* The Cross-Section Areas were (it is believed) computed from equidistant soundings chiefly by the Trapezoidal or Arithmetic Mean Rules: these give Results usually too small (Art. 13) in consequence of the general concavity of the bed.

20f. Discussion.—There is a pretty close agreement between the Results for the Kamhera Reach: but from the fact that the official Table for this Reach was only a provisional one, based on an *assumed constant slope*, and not on actual velocity-measurement, there can be little doubt that the fair agreement of the new Results therewith is accidental; and is no evidence of the correctness of either, the fact being that the surface-slope (which is what should be used in Bazin's formula) is certainly not constant when the state of Control is changed.

As to the other principal Reaches (Roorkee and Belra), there are three points of striking contrast between the Experiments' Results and the Canal Tables, viz.—

1°. The new Results are—at high water—uniformly much larger than those of the Canal Tables.

2°. At low water the new Results are extremely variable.

3°. At low water even the maxima of the new Results fall short of the Canal Tables.

The first Discrepancy may be explained by supposing either (1), that the new Results err in excess, or (2), that the Canal Tables err in defect, or (3), that both these errors exist. And the excess of the new Results might at first sight be taken as evidence of an excess error inherent in the use of Rod-velocities (without the application of a reduction-co-efficient), due to the foot of the Rod not grazing* the bed. But if this were really the case, the excess in question should be relatively much larger at low water, (because a small "Lift" of the foot of the Rod above the bed is a much larger fraction of the whole depth at low water than at high water,) whereas the excess *actually disappears at low water* (*see* 3° above).

On the other hand, there is strong reason (Art. 20e) to expect the High Water Results of the Canal Tables to err in defect.

Lastly, the excess of the low water figures in the Canal Tables over the maxima of the new Results at low water is probably due to the dependence of the former chiefly on high water Experiments (Art. 20b).

20g. VARIABILITY OF NEW RESULTS.—The variability of the new Results *at the same depth* is of course due to the variability of the velocity factor even with constant depth (Art. 17a, b), the Experiments having been so numerous as to be performed under greatly varied Conditions at certain depths. The dependence of the velocity on the External Conditions will be discussed in next Chapter.

21. Discharge-Tables.—Enough has been said to show that the mere depth of water at a Gauge is very far from being the only—possibly not the most—important element in the Discharge, so that a "Discharge-

* This objection has already been disposed of, *see* Ch. XV, 14b, & XVI, 10, 11.

Table" ought certainly to be *one of double entry*, (showing both the Gauge-Reading and Surface-Slope as Arguments.)

[The Ganges Canal Official Discharge-Tables purported to show a definite Discharge for a given Gauge-Reading. Such a state of things would seem to be possible only at a Site with permanent bed, and there only when the External Conditions were all *fixed for a given Gauge-Reading*, whereas in this Canal they are extremely variable (from the great powers of Control)].

22. Discharge-Formulæ.—Most Cubic Discharge-Formulæ as yet proposed are of the form **D = A . V**, exhibiting the Area and Mean Velocity as *separate factors*, so that in comparing Results computed from the formulæ with experimental Results, it *is sufficient* (and also more* convenient) to *compare only the Mean Velocities*. All such comparisons are deferred to next Chapter. Most formulæ of this type are really empirical, *i.e.*, the functional form has been *assumed* (by mere guess work, or with help of some admittedly imperfect Theory), and the co-efficients only have been fitted to agree with such Experimental Results as were available.

22a. Moseley's Discharge-Formula.—This formula is not of the usual type **D = AV**, so will be discussed at once. It has a special theoretic interest in being one of the few cases in which an attempt has been made to deduce a formula from a rational Theory of Fluid Motion. The expression proposed† for the Cubic Discharge in an Open Channel of any form is—with symbols changed to suit this Work—as follows :—

$$\text{Discharge in cub. mètres per sec., } D' = \tfrac{1}{2}\frac{B'}{R'} \cdot \left\{1-(1+2R') \cdot \epsilon^{-2R'}\right\} \cdot v_0',$$

where B', R' are expressed in mètres, and v_0' in mètres per second.

An attempt is made to verify this formula by comparing the numerical Results given by it with 46 of Bazin's Experiments in Open Rectangular and Trapezoidal Channels not exceeding 2 mètres (about 6′ 6″) in breadth, nor ½ mètre (about 1′·6) in depth. The Discrepancies are in most cases only small fractions of the Total measured Discharge, though in one (case No. 6, on p. 49 same Volume) the Discrepancy is 50 per cent. of the measured Discharge.

This general close agreement with Bazin's small scale Experiments turns out, however, to be a very poor Test of the formula, for when applied to large scale Results it fails so entirely, as to be *obviously faulty in form*.

This can be shown in a general manner (without taking the trouble of making the complete numerical computation) as follows :—

It is obvious that the quantity $(1 + 2R') \cdot \epsilon^{-2R'}$ is always a + quantity rapidly decreasing as R' increases. Hence the above expression gives a Result always less than $\tfrac{1}{2}\frac{B'}{R'}v_0'$ cubic mètres per second, (where B', R', v_0' are all expressed in mètres), and therefore the—

* In consequence of the Mean Velocities being comparatively small numbers.

† Moseley's "Steady Flow", Philos Maga., Vol. XLIV, p. 46, Eq. (65) or (67).

$$\text{Discharge (in cub. ft. per sec.) is always} < \frac{(3{\cdot}281)^2}{2} \times \frac{B}{R} \cdot \frac{v_0}{3{\cdot}281}$$

$$\text{or} < 5{\cdot}382 \frac{B}{R} \cdot v_0,$$

where B, R, v_0 are measured in feet.

Now applying this to the following cases from these Experiments :—

SITE.	Table.	Series.	DATA. B	DATA. R	DATA. v_0	Value of $5{\cdot}382 \frac{B}{R} v_0$, [should be > D]	Cubic Discharge Measurement. D
Soláni Left Aqueduct, ...	LVIII—LXX	106	100·2	6·75	4·05	324	2,328
Soláni Right Aqueduct, ..		108	105·8	7·96	4·52	323	3,429
Soláni Embankment Main Site,		151	190·8	9·34	4·66	512	7,169
Fifteenth Mile, New Site, ..		191	182·0	9·49	4·91	507	7,187
Belra Site, ...		201	196·4	9·02	3·67	430	5,611
Jaolí Site, ...		211	200·2	7·82	3·43	473	4,631
Kamhera Site, ...		221	69·5	4·84	3·40	263	961

It will be seen that the computed Results (values of $5{\cdot}382 \frac{B}{R} v_0$), which should be all *greater than* the Discharge-measurements (D), are actually *only from about* $\frac{1}{4}$ *to* $\frac{1}{14}$ *of the latter*, so that the formula is evidently useless. It is in fact only quoted as being one of the very few modern attempts to construct a formula for Discharge on a rational basis (*i.e.*, not purely empirical), and to show the danger of verification based solely on numerical comparison with small scale Experiments.

[This failure is probably connected with the failure of the Velocity-Curves deduced from the same Theory, pointed out in Ch. XVIII, 3c].

CHAPTER XX.

MEAN VELOCITY.

Preface.—This Chapter treats of Mean Velocity-measurement, (Art. 1, 2, 4—6, 12,) and its Variation, (Art. 3—3b, 13—16d,) and also contains a critical discussion of various rapid Approximations thereto, (Art. 7—12f, 17—29.) The most important Articles are (omitting the detail) Art. 1—8, 14, 15, 16c, 20a, 21, 22a,c, 23c, 26—29, (especially the last three.) The Conclusions (Results (45), (46), (55), (55a), (57), (58)) as to the various approximations are *of great practical importance.*

1. Mean Velocity, PAST TRANSVERSAL, and SECTIONAL.—It will be convenient to discuss these two sorts of Mean Velocity to a great extent together. They have already been defined (Ch. IV, 3) as follows:—

Mean Velocity past a Transversal. "The Mean of the 'forward velocities' at all points of a Transversal".

Mean Sectional Velocity. "The Mean of the 'forward velocities' at all points of a Cross-section".

It seems clear that their *proper* values are:—

$$\text{Mean Velocity past Transversal, } U = \frac{\text{Supfl. Discharge past Transversal}}{\text{Breadth of Transversal}} = \frac{D}{b}, \ldots (1).$$

$$\text{Mean Sectional Velocity,} \qquad V = \frac{\text{Cubic Discharge}}{\text{Area of Cross-Section}} = \frac{\mathbf{D}}{\mathbf{A}}, \ldots\ldots\ldots\ldots (2).$$

The importance of these quantities consists almost entirely in their use as a Step towards computing the Cubic Discharge.

[The only Transversals experimented on were those at Surface, Mid-depth, and Bed; when *necessary to distinguish* these the symbol U is written U_0, $U_{\frac{1}{2}H}$, U_H respectively].

2. *Arithmetic Mean too small.*—For the same reasons as in Ch. XIV, 2, *q.v.*, the Arithmetic Mean of the velocity-measurements (of each kind) past the several verticals right across the channel, would give—an approximation of course, but—*usually too small* a result.

[This course seems to have been used in the Mississippi Report (*see* Tables, pp. 237, 240, 242 thereof) in the case of the 5-foot depth Transversal].

3. Mean Velocity Variation small.—It is clear that the Mean Velocity past a Transversal, and the Mean Sectional Velocity being the means respectively of all velocities past the Transversal or through the Cross-section, their variation must be some sort of *mean of the* variations of the individual velocities, so that in fact—

"The Mean Velocity past a Transversal, and Mean Sectional Velocity are less variable from instant to instant than most of the individual velocities of which they are the mean",..(3).

[An examination of the line δ of "Ranges" of the velocities throughout Det. Tab. XXIX—XXXIII, and XXXIV—LVI will amply confirm this].

3a. *Mean Velocity past a Transversal not constant.*—It is indeed quite possible that the variations of the individual velocities past a Transversal might nearly balance, so as to leave the Mean Velocity past a Transversal nearly* constant. As far as can be judged from the present Experiments this is not the case, or in other words—

"The Mean Velocity past a Transversal varies sensibly from instant to instant",..(4).

[A glance down the Column of U in Tab. XXIX—XXXIII will show at once considerable variations in magnitude even in successive SETS in the same SERIES. The AVERAGE value and "Range" of U in each Series are given in Abstr. Tab. 13: the Ranges will be seen to be *pretty large fractions* of the whole quantity, even as high as ·63 in 4·06, or 15·5 per cent., (Ser. 58.)

This evidence is not so strong as might appear at first sight, inasmuch as *much of this variation* can be traced to Change of External Conditions. Perhaps the only fair Test is the comparison of *successive Results of the same day's work*, in which it may be fairly assumed that the External Conditions (except Wind) remained tolerably constant. There are many cases of this sort (viz., of several Sets of work done in one day) throughout these Tables, the discrepancies between which are so great as to fairly establish the above Result].

3b. MEAN SECTIONAL VELOCITY CONSTANT.—It being assumed (Ch. XIX, 18) that the Cubic Discharge is constant from instant to instant, so long as the Conditions remain constant, *i. e.*, with constant water-level, and constant state of Supply into and Withdrawal from the Reach producing constant Surface-Slope, it follows that (the Area thus also remaining constant)—

"The Mean Sectional Velocity is constant from instant to instant",..........(5),

and is thus not subject to the instantaneous variation which the velocities at individual points undergo, or in other words is technically STEADY: it is thus unnecessary to speak of an AVERAGE value of it in the sense in which the term AVERAGE VELOCITY has hitherto been used.

But whereas the Cubic Discharge is directly affected by every change of water-level, from the consequent change of depth, such changes do not affect the Mean Velocity when the change of Conditions is insufficient to affect the velocities generally, so that—

"The Mean Velocity is constant in a higher degree than the Cubic Discharge",(6).

* This opinion was hazarded—as to Mean Surface Velocity—in the 1874-75 Report, (Art. 30,) but it seems more probable now that the evidence was insufficient.

[Great use will be made of this property (5) in the sequel (Chap. XXI) in testing the accuracy of the work. It may be objected that the Experiments apparently contradict this, in that the Mean Velocity-measurement (**V**) figured in Tab. XXXIV—LVI varies considerably in magnitude even in successive Sets of the same Series, and in some cases *even in successive Sets of the same day*. *See* Art. 5 below as to one chief cause of this: the Discussion is however a wide subject, and occupies the next Chapter. Meanwhile it may be noted at once that the Tables at any rate show the variation of Mean (Sectional) Velocity-measurement to be less than that of the details (Mean Velocities past many verticals in the cross-section) from which they were obtained, (compare Result (3) above)].

4. Present Mean Velocities.—The Discharges (past Transversals and Cubic) computed separately for each SET of velocity work, and shown in Col. 7 of Tab. XXIX—XXXIII and XXXIV—LVI, enable the Mean Velocity of each kind (U, **V**) to be computed separately for each SET by the proper formulæ $U = D \div b$, $\mathbf{V} = \mathbf{D} \div A$. This has been done separately for each SET shown in the Tables, and the Result is shown in Col. 8. And since the Discharges of each kind have been in every case* computed by the best approximation-formulæ extant, the Mean Velocities in question are the *best approximations* to the true values that *could be obtained from the data.*

[*Soláni Aqueduct Sites.* It has been explained (Ch. XIX, 5a & 8b) that the Cubic Discharge was computed (for these Sites) as if the cross-section were truly rectangular (neglecting therefore the slight contraction at the Bed and also under the corbelling) by the formula—

$\mathbf{D}$ = Area of Mean Velocity-Curve (through full breadth b') × Full Depth $= D_m H$.

In computing the Mean Velocity, the Area (A′) was similarly taken as if truly rectangular, (and not from Tab. VI which contains the true values,) *i.e.*, $A' = b'H$: thus $\mathbf{V} = \mathbf{D} \div A' = D_m H \div b'H = D_m \div b'$, so that the *practical* Rule was—

$$\mathbf{V} = \frac{\text{Area of Mean Velocity-Curve (through full breadth } b')}{\text{Full breadth } b' = 85 \text{ feet}}\Bigg] \quad \text{............(7).}$$

5. Mean Velocity-Error.—The Mean Velocity-measurement of each kind (U or **V**) obtained as above is of course liable to error similar to the Discharge-measurement of same kind (D or **D**) from which it is derived. But, as explained in Ch. IV, 4a, the effect of errors in estimation of the several Average Depths and Average Breadth is almost wholly eliminated, so that the residual error is sensibly only that due to error in the primary Velocity-measurements on which the Discharge-measurement D or (**D**) depends.

This Error is of the same character as, and is also proportional to, the similar error in the Discharge-measurement, since $U = D \div b$, and $\mathbf{V} = \mathbf{D} \div A$. Thus, in consequence of the Unsteady Motion, a single Mean Velocity-measurement (*i.e.*, that obtained from a single Set) cannot be considered a good, but only a FAIR value.

* Except perhaps in the Side-Spaces, (Ch. XIX, 7c,) but the Error produced in the Mean Velocities is quite trifling, (Art. 7.)

[It will be understood that, in the case of the Mean Sectional Velocity, it is not the Mean Velocity itself that is subject to variation from instant to instant, but that the measure thereof (*i.e.*, the Mean Velocity-measurement) is an imperfect one, in consequence of being derived from non-simultaneous Rough Velocity-measurements at numerous points].

6. Practical Bearing.—The practical use proposed to be made of the value of the Mean Sectional Velocity obtained as above is partly as a Test of the correctness of the work itself (Ch. XXI, 2—4c), partly as a Test of the sufficiency of the approximation of certain other modes of *rapidly* obtaining an approximate value thereof (Art. 20b, *et seq.*).

[Very little use will be made of the Mean Velocity past a Transversal: it is printed rather for the sake of completeness, and of possible future use, than of immediate use].

7. Approximation, IMPORTANCE OF.—It is obvious that if an approximate value of the Mean Velocity of either kind (U, V) could be obtained by any *rapid* process in the Field, it would serve far better for practical purposes for calculation of the Discharge (of that kind) by the fundamental formulæ—

$$D = U \cdot b, \quad D = A \cdot V, \quad \text{..............................} \quad (8),$$

than the process detailed in Ch. XVII, XIX depending on the tedious Field-work of velocity-measurements at many points in the Cross-section.

8. Mean Sectional Velocity, V.—This quantity is of such great practical importance, that an immense amount of labor has been bestowed in the search for some rapid approximations to it. These approximations have taken two chief forms, one involving only Surface-Slope and Cross-section data, the other only Velocity data, and therefore requiring very different Field-work, viz.,

1°. Surface-Slope, and Cross-Section Measurement.

2°. Velocity-Measurements, (a few only.)

9. RELATION OF V, $\sqrt{RS}$.—The expression of the relation between V and S and quantities such as *b*, B, H, R, &c., depending only on the Site by which the Mean Velocity could be computed from Surface-Slope measurements (without resorting to velocity-measurement) has long been an object of research. A very good summary of the attempts that have been made at various times up to about 1858 is given in the Mississippi Report, pp. 207—220. In most of these V is made a function of the product RS. The earliest is Chézy's—

$$V = C \cdot \sqrt{RS}, \quad \text{....................................} \quad (9),$$

in which **C** is a numerical co-efficient to be determined by experiment, which was at first *supposed to be constant* for each Site. Various numerical values were assigned to this co-efficient from time to time by different writers, as below, (p. 208, *op. cit.*)—

Young, 84·3 ; Eytelwein, 93·4 ; D'Aubuisson, 95·6 ; Downings & Taylor, 100 ; Leslie, 68 & 100 ; Beardmore 94·2 ; Neville, 92·3 & 93·3 ; Stevenson, 69 & 96,

each no doubt suited to the particular Experiments discussed. As the data available increased, it became evident that in the Chézy formula no single constant value of **C** was of any general applicability, and that if the formula be used at all, it *must be used with a variable co-efficient.*

After various trials of other functional forms—some of which will be discussed further on—some of the more modern writers (*e.g.*, Messrs. H. Bazin and W. R. Kutter) have reverted to the older form, and endeavored to construct a formula for the value of the co-efficient **C** in terms of the cross-section data (R, &c.) and surface-slope. It seemed therefore worth while giving this formula (with variable co-efficient) an extended trial.

From the excessive smallness of S (rarely exceeding ·0005) in Open Channels—when not torrents—the quantity $\sqrt{RS}$ is inconveniently small for tabulation : the quantity $100\sqrt{RS}$ is on the whole far more convenient (for tabulating), and is indeed in many cases itself a *rough approximation* to the Mean Velocity, and is in all cases—as far as yet known—a quantity at any rate comparable (in magnitude) with it. For these reasons, the Chézy formula is written in this Work in the modified* form—

$$\mathbf{V} = \mathrm{C} \times 100\sqrt{\mathrm{RS}}, \text{ or } \mathbf{V} = \mathrm{C} . w, \text{ where } w = 100\sqrt{\mathrm{RS}}, \qquad (10),$$

and the quantity $w = 100\sqrt{RS}$ *will be treated as a* (computed) *velocity.*

[The experimental values of **V**, and of the co-efficients connected will first be discussed, (Art. 10—19b,) after which the subject of approximation to value of **V** by formulæ will be taken up again (Art. 20).

10. Experimental Research.—In these Experiments data have been obtained on a very extensive scale for *three* rapid approximations to the Mean Sectional Velocity. In fact the whole Experiments may be said to have been directed to this end, all that preceded being preliminary investigation leading up to it: it was throughout the *one important practical object kept in view.*

The three approximations in question depend on the following Field-work, viz., measurements of—

1°, Central Mean Velocity (U_o) ; 2°, Central Surface Velocity (v_o) ;
3°, Surface-Slope (S),

* suggested in Jackson's "Canal and Culvert Tables", 1878, p. 8.

the approximation being intended to be made by the application of certain "reduction-co-efficients" which will be denoted by c, c, C, thus—

$$1°,\ \mathbf{V} = c \, . \, \mathrm{U}_0\,;\quad 2°,\ \mathbf{V} = \mathrm{c} \, . \, v_0\,;\quad 3°,\ \mathbf{V} = \mathrm{C} \, . \, w, \ldots\ldots (11).$$

The experimental research becomes in fact ultimately the preparation of *experimental values* of the co-efficients c, c, C computed from the experimental values of **V**, U_0, v_0, w by inversion of the fundamental equations above, viz.,—

$$1°,\ c = \mathbf{V} \div \mathrm{U}_0\,;\quad 2°,\ \mathrm{c} = \mathbf{V} \div v_0\,;\quad 3°,\ \mathrm{C} = \mathbf{V} \div w, \ldots\ldots (12).$$

[Note that the symbols U_0, v_0 are used in this Chapter with two distinct meanings each, according to the style of velocity-work discussed, thus—

Velocities past a Transversal, (*Surface, Mid-depth or Bed*),	v_0	denotes	Central Velocity (on the Transversal).
	U_0	"	Mean Surface Velocity.
Mean Velocity work,	v_0	"	Central Surface Velocity, (Art. 12a.)
" "	U_0	"	Central Mean Velocity, (Art. 11.)

The context will generally show which is meant, thus: in connexion with **V** they always have the latter meanings].

10a. RECORD IMPERFECT.—It was unfortunately impossible to obtain the data of *all* four quantities **V**, U_0, v_0, S complete on every occasion; stress of weather (and sometimes other causes) often interfered. Thus high wind affects the surface of the water so much that, when these systematic Experiments were first begun, it was thought undesirable to measure the Surface-Slope when the air was not almost calm, or the Central Surface-Velocity when the wind was high. The Discharge-measurement Field-work (which includes both **V** and U_0, *see* Ch. XVII, 7) was on the other hand carried on constantly even in pretty high wind. Thus—

"With every **V** there is a U_0 available, but the numbers of v_0, S available are far fewer".

Thus the data for the ratio $c = \mathbf{V} \div \mathrm{U}_0$ *include the whole* of the SETS of Det. Tab. XXXIV—LVI, whilst those available for the ratio $\mathrm{C} = \mathbf{V} \div w$ are far fewer, and those for the ratio $\mathrm{c} = \mathbf{V} \div v_0$ are fewer still. For this reason they could not be conveniently arranged in a single Tabular Form. The data for the ratio c have accordingly been abstracted into one group of Tables **14—18** (*see* Art. 11a), whilst the data for the ratios c, C have been combined into the Comparison Tables LVIII—LXX, and **20—22** as explained below (Art. 12c,e).

[This deficiency of the record of v_0, S affects the Roorkee Reach chiefly; it scarcely affects the work in the Belra, Jaolí, and Kamhera Reaches at all. For details of this as far as concerns S, *see* Ch. VII, 8—8c].

10b. MEAN VELOCITY PAST A TRANSVERSAL, U.—For the sake of completeness a "reduction-co-efficient"* (c) similar to the above has been computed connecting this Mean Velocity (U) with the Central Velocity (v_0) past the same transversal, so that—

$$\mathrm{U} = \mathrm{c} \, . \, v_0, \ldots\ldots (13),$$

although but little use has been made of it, (Art. 6.)

* To preserve the analogy of notation of co-efficients above, the symbol used for this co-efficient should have been c, not c. Tab. 13, and Pl. XLIII containing it were printed off before this was noticed. This is of little importance, as the "co-efficient" is not discussed below.

11. Central Velocity, (v_o, U_o).—This quantity has been defined (Ch. IV, 3) as the—

"Forward velocity past the centre of any Transversal",

the most important one being of course the Central Mean Velocity (U_o), or Mean Velocity *past the centre vertical.*

In the systematic measurement of velocities past a Transversal (as surface, mid-depth, bed, or mean), the velocity at centre (v_o or U_o) was always measured as part of the regular Field-work (Ch. XVII, 7), and is entered in the centre Sub-Column of Col. 8 of all the Detailed Tables of such work, XXIX—XXXIII (for v_o), and XXXIV—LVI for U_o.

[Each such Central Velocity-measurement is therefore—by the custom of repeating each observation thrice—the mean of only three distinct observations, or what has been called a *Rough Velocity-measurement*; whereas every Mean Velocity-measurement has been explained to be a *Fair measure* of that quantity. Thus the two terms entering into the value of the ratio are of very unequal precision. This could only have been obviated by repeating every Central Velocity-measurement 50 times, so as to secure its *Average value.* Unfortunately the idea of using this ratio did not occur to the Author till after the Field-work was closed].

11a. Abstract Tables, (13—18).—For ready reference Abstract Tables of the chief data and Results have been formed as follows :—

Surface-, Mid-depth- and Bed-Velocity work,	Abstr. Tab. 13, from Det. Tab. XXIX—XXXIII.
Mean Velocity work,	Abstr. Tab. 14—18, from Det. Tab. XXXIV—LVI.

Each Series containing more than one Set is summed up in two lines as follows :—

The upper line shows the Mean Results of the Series.

The lower line (in old brevier type (·18)) shows the "Ranges" of the details.

The details abstracted are (those on which the ratio of Mean to Central Velocity may be supposed to depend) the following for each Series :—

Number of Sets, Material (and sometimes Length) of Instrument.

Means and Ranges of h or H, R, b, F_1, F_2, F_3, S, and Mean Wind.

Means and Ranges of v_o, D, U in Tab. 13, and of U_o, **D**, **V** in Tab. 14—18.

Lastly, the Mean value of the ratio $\mathbf{c} = U \div v_o$, or $c = \mathbf{V} \div U_o$ has been computed for each Series from the above mean values of U, v_o; **V**, U_o, (not from the details) and is given in Col. 8.

These Tables permit of the rapid comparison of the Average values of the Mean and Central Velocities, viz., of U, v_o, **c** and of **V**, U_o, c; and reference to them will often save reference to the Detailed Tables.

[It would undoubtedly have been better to have given the values of the ratios **c**, c *in detail for every Set* throughout the Detailed Tables XXIX—LVI; this could have been done with one extra Column throughout: unfortunately the desirability of this was not seen till these Tables were printed off].

12. Field-work, (for **V**, U_o, v_o, w; c, **c**, C).—In order to obtain good experimental values of the co-efficients c, **c**, C, it is clearly necessary that the Field-work on which they depend, viz., Measurement of—

Cubic Discharge (D), for obtaining the fundamental **V**,
Central Mean Velocity (U_0), Central Surface Velocity (v_0), Surface-Slope (S).
should be executed under closely similar External Conditions. With this view the Field-work of all four was always done *in as rapid succession as possible*, and usually in one of following orders:—

i. S, **D**, v_0; ii. S (L. Bank), **D**, v_0, S (R. Bank).
iii. S (L. Bank), Half of **D**, v_0, Half of **D**, S (R. Bank).
iv. S (L. Bank), Half of **D**, v_0, Half of **D**, S (R. Bank).

Also a Central Mean Velocity-measurement (U_0) was necessarily made at the middle of every Discharge-measurement (as part of the Field-work thereof, Ch. XVII, 7).

[It will be seen that a Slope-measurement usually preceded the rest: this was to take advantage of the early morning hours which are usually the calmest; but if there was a high wind at first, the Slope-measurement was often put off on the chance of the wind falling. Similarly the Central Surface Velocity-work was sometimes advanced to take advantage of a calm time of day].

12a. *Central Surface Velocity-measurement*, (v_0).—When it is proposed to make the Cubic Discharge-measurement depend on the velocity at a single point, it is obviously important to obtain a really good measure of the Average value of the latter.

Accordingly every Central Surface Velocity-measurement (which was intended for use in this way) was *repeated* 48 *times* (24 times by each Observer as Time-keeper): the mean of these is therefore an Average Central Surface Velocity-measurement, in the sense of Ch. VI, 5, freed of the Observers' relative personal equations; these AVERAGE values are the *only quantities printed* for comparison with the Mean Velocities, (all the details from which they were obtained being omitted,) *see* Col. 4 throughout Tab. LVIII—LXX.

The Gauge-Reading and state of the Wind were always noted just before and just after each SET of 48 such measurements, so that each such piece of work is a SET complete in itself in the sense used in Ch. VI, 12, *q. v.*

[It will be seen that as both terms of the ratio $\mathbf{V} \div v_0$ depend on about the same number of distinct velocity-measurements, *each is of about equal precision*].

12b. *Surface-Slope Measurement*, (S).—For detailed explanation of this, *see* Ch. VII, 2d, e, 3 & 8—8c. For convenience of printing, the abbreviation w has been introduced for the expression 100 $\sqrt{RS}$.

[There is some doubt as to the relative precision of the two terms of the ratio $\mathbf{V} \div w$. From the delicacy of the Surface-Slope measurement (Ch. VII, 2b), it appears to the Author that the term w *cannot be expected to be of nearly equal precision with* **V**].

12c. CO-EFFICIENTS c, C, (Comparison Tables LVIII—LXX).—The data for ready comparison of the three quantities **V**, v_0, w, and for computation of their ratios ($c = \mathrm{V} \div v_0$, $C = \mathbf{V} \div w$), have been collected together in Tab. LVIII—LXX: these will be styled DETAILED

COMPARISON TABLES to distinguish them from the preceding Detailed Tables XXXIV—LVI.

They are divided into 5 Main Columns, each containing the whole of the data of one sort of work. Thus—

Col. 1 contains the date, (used for reference.)
Col. 2 „ „ data showing the state of Control, and Surface-Fall.
Col. 3 „ „ „ for the Rod-Velocity Result $\mathbf{V} = \mathbf{D} \div \mathrm{A}$.
Col. 4 „ „ „ „ Central Surface-Velocity, (v_o,) *see* Art. 12a.
Col. 5 „ „ „ „ Surface-Slope Result, w, *see* Art. 12b.

For explanation of the details in each Main Column, *see* Vol. II, p. 115 (the page preceding the Tables in question).

Each line of these Tables shows a SET of various data collected *in as rapid succession as possible,* (Art. 12,) *complete under at least two of the three heads* $\mathbf{V}, v_o, w$, (Cols. 3, 4, 5), viz.—

$\mathbf{V}$, (Col. 3,) complete throughout.
v_o, (Col. 4,) incomplete at Soláni Sites.
„ „ complete (with trifling exceptions) at the other Sites.
w, (Col. 5,) complete (with one exception, Ser. 195) at all the chief Sites.

Thus Col. 3 is an Abstract of the chief Data and Results, viz.—

R, Variation of water-level, b, l, Wind, Timekeeper, $\mathbf{D}$, $\mathbf{V}$,

from Det. Tab. XXXIV—LVI, viz., *for those Sets only for which the data for either* v_0 *or* w *were also collected,* (as above shown.)

The experimental values of the ratios c, C are given on the right hand of Cols. 4, 5, *separately for every Set* (in which the data are available).

Such Sets as were done at nearly same water-level, and do not differ greatly in the Mean Velocities ($\mathbf{V}$), &c., are grouped together into one Series, nearly as in the Detailed Tables. In a few cases only some of the Series of the Detailed Tables have been combined or broken up into smaller Series: but in every case the Series in these Tables bear the same Serial Nos. as in the Detailed Tables to enable ready reference to be made.

12d. ARGUMENT, *Hydraulic Mean Depth.*—The entry of the hydraulic mean depth (R) being essential in these Tables, (at any rate in Col. 5 for the computation of the quantity $100\sqrt{\mathrm{RS}}$,) it seemed to be the quantity most convenient to choose as the leading entry or "Argument" of each of the Col. 3, 4, 5.

From the length of time taken in each separate kind of work, (Slope-measurement, Rod-velocity work, Central Surface-Velocity work,) it often happened that the *water-level changed in the course of the work* in such a way that the Average Water-Level (and therefore also the Hydraulic Mean Depth) of each kind of work done in succession according to the scheme of Art. 12 differed somewhat: it has therefore been necessary to give the hydraulic mean depth (R) separately for each kind of work in Col. 3, 4, 5.

[The difference of hydraulic mean depths in question seldom exceeded ·02, but it has occasionally amounted to from ·08 to ·16 when the Canal was not in train, *see* Ser. 119, 124, 137, 138, 153, 155, 158, 174].

The Rod-velocity work (Col. 3) has been considered to give the fundamental values of Mean Sectional Velocity. For this reason the several SETS of each Series

have been arranged generally according to decreasing hydraulic mean depth of Col. 3, which therefore forms the ARGUMENT of these Tables.

[The Gauge-Readings not appearing essential in this inquiry have not been printed in these Tables. They can be found at once, however, by comparison with Tab. XXXIV—LVI by inspection for Col. 3, and by applying the change of hydraulic mean depth as a correction for Col. 4, 5].

12e. ABSTRACT COMPARISON TABLES, (Tab. 20—22).—These Tables have been formed for the sake of ready reference from the Detailed Comparison Tab. LVIII—LXIX.

Each Series containing more than one Set is summed up in two lines as follows:—

The upper line shows the Mean Results of the Series.

The lower line in old brevier type (·18) shows the Ranges of the details.

The details shown are the same as in the Detailed Comparison Tables, *see* Vol. II, p. 115 (preceding those Tables); with following additions:—

The Numbers of Sets of each kind of work, under heads **V**, w (Col. 3, 5), and v_0 (Col. 4) are figured separately in Col. 1, 4 respectively.

The values of the ratios c_b, C_b, C_k of Bazin's and Kutter's formulæ (explained below, Art. 21—23) are printed beside the experimental values.

These Tables permit of the rapid comparison of the Average values of **V**, v_0, w, c, C: reference to them will often save reference to the Detailed Tables.

12f. *Record of v_0 incomplete.*—From the incompleteness above noted of the record of v_0 (especially at the Soláni Sites), it follows that—although the values of **V**, v_0 w, c, C *of the same Set* are of course always fairly intercomparable—the Ranges and Means of v_0, c *of a whole Series* are often (especially at the Soláni Sites) not fairly comparable with those of **V**, w, C, viz., whenever the latter are obtained from a larger number of data than the former, with conditions perhaps varied. These cases are marked as follows:—

Det. Comp. Tab. (LVIII—LXIX).—In all such cases the Ranges and Means of the Main Column 4 are queried (?); and the Number of Sets figured on left hand of Table (as "Means of 6", &c.) must be held to apply only to Col. 3, 5.

Abstr. Comp. Tab. (20—22).—The Numbers of Sets available under the heads of **V**, w, (Col. 3, 5) and v_0, (Col. 4) are figured separately in Col. 1, 4.

13. Velocity-Variation, DIAGRAMS, (Pl. XLIV—XLIX).—In order to trace readily the mutual dependence of the principal velocities (U, v_0; **V**, v_0, w), and also their dependence and that of the ratios c, C on the External Conditions, such as Depth, Control, Surface-Fall, Surface-Slope and Wind, Diagrams have been formed—

Central and Mean Surface-Velocity-Variation (v_0, U_0), Pl. XLIV.

Central Surface and Mean Velocity-Variation (v_0, **V**), Pl. XLV—XLIX.

[So few data (only two Series each) were available for the Mean Mid-depth and Mean Bed Velocity ($U_{\frac{1}{2}H}$ and U_H) Variation, that it was not thought worth while publishing similar Diagrams for them].

These show the velocities (U_0, v_0; **V**, v_0, w), the Discharges (D, **D**) on which U_0, **V** depend, the ratios (c, C), the Surface-Falls (F_1, F_2, F_3), Surface-Slope (S), Average Obstruction (k) at Tail of Reach, Withdrawal by Distributaries (Q)—(or as

many of these data as were available in each case)—plotted as ordinates to the hydraulic mean* depths (R) taken as abscissæ.

The state of the Wind is plotted as explained in Ch. V, 21d.

The plotted points of any one kind have been joined usually by straight lines (or by curved lines if necessary for clearness) between successive ordinates, thus forming a sort of Curve of each quantity, made up of short straight lengths (and thus free from any bias of the draughtsman). For clearness' sake these Curves are drawn in different styles of clear and dotted lines. The Curves are drawn continuous only through actual plotted points on these ordinates; wherever a plotted point is wanting, the Curve is broken off at the crossing with that ordinate: breaks in the ordinate-lines are made for sake of distinctness whenever the curves cross them at very acute angles. The same scales have been used as far as possible throughout.

For full details of symbols, scales, styles (of Curve), &c., *see* the Explanations on Pl. XLIV, XLV, and Notes on each Plate.

The diagrams have been prepared from following data:—

Pl. XLIV, Surface Velocity-work, from Abstr. Tab. 13.

Pl. XLV—XLVIII, Mean Velocity-work, from Abstr. Comp. Tab. 20—22.

Pl. XLIX, Mean Velocity-work, from Det. Comp. Tab. LXIX.

Thus these Plates show Mean or Detailed Results as follows:—

Pl. XLIV—XLVIII show only Means, *one ordinate being allotted to each Series.*

Pl. XLIX shows Details, *one ordinate being allotted to each day's work,* (*i.e.*, usually to each SET, or to the mean of two Sets done on same day).

[To enable the relations between $\mathbf{V}$, U_0 and the dependence of U_0, c on the External Conditions to be traced out equally readily, curves of U_0, c should have been superposed on all these Diagrams, or else a special Set of Diagrams should have been prepared. The present Diagrams were, however, printed off before it was decided to include the quantities U_0, c in this Research, and after drawing out in MS. the special Diagrams required, it did not appear worth while to publish them; they cover of course the same space as the present ones].

14. Velocity-connexion.—The Abstr. Tab. 13, 14—18, 20—22 show that—

"There is *a general sort of agreement* in the variations of U, v_0; $\mathbf{V}$, U_0; $\mathbf{V}$, v_0, w, their increase and decrease being generally concurrent",................(14).

The Plates (XLIV, and XLV—XLIX) also show this at once for U_0, v_0 and for $\mathbf{V}$, v_0, w, by mere inspection of the curves, the larger saliences and depressions occurring generally on the same ordinates; this agreement, however, by no means extending uniformly to details.

This departure from close agreement is also rendered evident by the irregularity of the variation of the ratios c, $\mathbf{c}$, C in the Tables, and by the irregularity of the curves of $\mathbf{c}$, C in the Plates.

15. Velocity-variation.—A mere glance down the U, v_0; $\mathbf{V}$, U_0; $\mathbf{V}$, v_0, w Columns of Abstr. Tab. 13, 14—18, 20—22, and at the U_0, v_0 and $\mathbf{V}$, v_0, w Curves in Pl. XLIV & XLV—XLIX will show at once that—

* In conformity with the Rule (Art. 12d) for the Comparison Tables, the hydraulic mean depths of the Rod-velocity-work—which form the "Argument" of the Tables—have been adopted as Abscissæ in Pl. XLV—XLIX.

"The velocities (U, v_0; V, U_0, v_0, w) decrease generally (in absence of other influences) with decrease of depth",..(15).

[This is especially evident at those Sites where the Range of Depth was greatest, *see* Abstr. Tab. 15—17, and Pl. XLVI, XLVII].

It is equally clear that this decrease is *very irregular*, so that the velocities must obviously depend *in an important manner* on some other elements, say on the Surface-Gradient, state of Control, Wind, &c. The Plates show a very general concurrence of the larger saliences and depressions of the Curves of velocity (U_0, v_0 and V, v_0) with some one or other of those of Surface-Gradient, (F_1, F_2, F_3, S), and contrariwise with the depressions and saliences of the Curve of Obstruction at Tail (k), so that it may be concluded that—

"Increased Surface-Gradient generally increases velocity",......................(16),

and

"Increased Obstruction at Tail generally decreases velocity",..................(17),

[*see* especially Ser. 131 to 139, Pl. XLV; Pl. XLVI & XLVII, *passim*].

Inasmuch as the Surface-Gradient is itself determined by the state of Control, it seems admissible to view the velocity as dependent primarily on the Surface-Gradient, and only mediately (*i.e.*, through the Gradient) on the State of Control. Summing up then, it may be said to be clear that—

"The velocities (U, v_0; V, U_0, v_0) increase with increase of either Depth or Surface-Gradient, and decrease with decrease of either",.........................(18).

Moreover, the Diagrams show that the change of velocity due to change of Surface-Slope is liable to be as great or even greater than that due to change of depth: so that *the state of the Surface-Slope* is sometimes a *more important element* in determining velocity than the depth of water.

[This has an important practical bearing in showing that Discharge-Tables must necessarily be of double entry, *see* Ch. XIX, 21].

16. Wind-effect.—It is by no means easy at first sight to trace the effect of Wind in accelerating or retarding velocity in the Diagrams. In the first place it is clear that only the up-stream and down-stream portions of the wind can be expected to have any *direct effect* in this way.

On examining Pl. XLV—XLVIII which, it will be remembered, show only "Mean Results", it will be seen that—except at the Kamhera Site (Pl. XLVIII)—there are very few cases of high up-stream or down-stream wind.

[The fact is of course that up- and down-stream winds were not the prevailing winds at any Site except the Kamhera Site, and further that such up- and down-stream winds as did occur frequently neutralize each other in great part in forming the Mean Wind (Ch. V, 21c) of a Series. Again, the few cases that actually do occur (out of the Kamhera Reach) are mostly cases at low-water when there was obstruction applied at Tail of Reach, a disturbing element quite sufficient to wholly mask the small effect of Wind, *see* Pl. XLVI, Ser. 122, 127; Pl. XLVII, Ser. 171, 181].

The Kamhera Reach, however, will be seen (Pl. XLVIII) to supply several cases of pretty high down-stream (about NNW.) wind: but on examining the curves of V and v_0 thereon, *no effect whatever is visible.* It would appear probable then that

the effect (if any) must be small, and is probably *lost sight of in the Mean Results* of Series.

16a. Wind-effect, Kamhera Reach, (Pl. XLIX).—The only way of tracing the effect appears then to be to compare (not the Mean Results of Series but) the separate Results of each day's work.

With this view Pl. XLIX has been prepared, showing the *separate Result of each day's work* for the Kamhera Reach taken from Det. Comp. Tab. LXIX. The state of the Wind is shown at both beginning and end of each Set by blunt arrows (————[) in the former, and by sharp arrows (————➤) in the latter case (Ch. V, 21d).

[In order to show each day's work distinctly, it was necessary to greatly increase the scale of abscissæ to $\frac{1}{10}$ foot to an inch (so as to spread out the ordinates), and on the other hand to reduce the Wind-scale to 40 feet per sec. to an inch (to confine the Diagram within the page). Similar detailed Diagrams were drawn out in MS. for the *whole* of the work in the other Reaches, but have not been* published].

16b. Wind-effect, Discussion.—In examining this Diagram, the large scale of hydraulic mean depths must be carefully remembered: the whole Range of this element will be seen to be only ·91, *i.e.*, from 4'·91 to 4'·00. There will be seen to be small irregularities throughout in the curve of **V**, and *much larger ones* throughout in the curve of v_0: moreover, the saliences and depressions of these two curves are to a great extent concurrent on the same ordinates, and are therefore presumably generally due more or less to the same causes.

And among these causes, it is clear from the Diagram that the Wind is a very efficient one in accelerating or retarding the Central Surface-Velocity.

Let the curve of v_0 be compared throughout its extent with the corresponding Wind (plotted from the *upper* Wind-Zero Line): it will be at once apparent that almost every high value v_0 corresponds to a high N. Wind (or wind with high northing in it), and almost every low value of v_0 to a low N. wind, or calm, and conversely; and that the only case of S. wind (or wind with partial southing) corresponds to a low value of v_0, (see the ordinate to abscissa $R = 4'·40$).

It by no means follows that these concurrences are necessarily wind-effects. Many of these cases are no doubt *partly*, (or perhaps wholly,) ascribable to the state of the Surface-Gradient.

[Throughout the range $R = 4'·40$ to $R = 4'·00$ (with two striking exceptions, viz., when $R = 4'·16$, and $4'·08$), the saliences and depressions in the curves of v_0 and S are generally concurrent upon the same ordinates: in many of these cases the variation of S seems too small to account wholly for the large change in v_0, so that the Wind (whose action chances to be concurrent with that of the Surface-Slope) is probably an efficient cause herein also; and, in the exceptional cases (of $R = 4'·16$, $4'·08$), is obviously so].

Throughout the range $R = 4'·91$ to $R = 4'·42$, there are frequent cases of saliences in the curve of v_0 with depressions in the curve of S along with wind with high northing in it. In these cases the wind seems to have been efficient in increasing the central surface-velocity in spite of the low Surface-slope.

* The Diagrams for the Soláni Aqueduct and Embankment Sites would be 8' and 9½' long on same scale: so that their publication would have greatly increased the cost of this Work. The Diagrams of Mean Results, Pl. XLIV—XLVIII, and the single Detailed Diagram Pl. XLIX, are considered to afford sufficient graphic evidence.

The Wind-effect on the Mean Velocity is on the other hand, as might be expected, somewhat obscure: most of the irregularities in the Mean Velocity-curve follow in fact those of the Slope-curve, so that the number of residual cases clearly ascribable to wind-effect is small, and the magnitude of the effect is also very small.

[The only tolerably marked cases are as follows:—

Ascribable apparently to wind only, see R = 4'·54, 4'·42, 4'·16.

Ascribable apparently partly to wind, (partly to slope,) see R = 4'·46].

16c. Wind-effect, General Conclusions.—The following General Conclusions seem fair:—

"The Surface Velocity-measurement is liable to be markedly retarded (under-estimated) by high up-stream wind, and accelerated (over-estimated) by high down-stream wind",..........(19).

"The Mean Velocity-measurement is only slightly affected—if at all—by high up- or down-stream wind",..........(20).

It seems very probable, moreover that, in consequence of the liability of the Surface-Floats and of the Rods (or rather of the portions of them projecting above the water) to be caught by the wind—

"The Surface and Mean Velocity-measurements are more affected by the wind than the real Surface and Mean Velocities",..........(21).

This is probably especially the case with the Surface Velocity.

[As already remarked (Ch. XII, 4) the chief primary effect of Wind (which is not of long duration) seems to be the production of ripples and waves, time being essential to the production of any marked translatory effect].

From the above may be drawn the following important practical Conclusion:—

"Discharge-measurements depending on Surface Velocity-measurements are liable to be markedly under- and over-estimated in high up- or down-stream wind", ..(22),

and as a consequence—

"Surface Velocity-measurements made in high up- or down-stream wind are quite unsuitable data for Discharge-computation", ...(23).

16d. Cross-Wind.—The effect of a cross-wind (*i.e.*, one blowing across the channel) upon either the velocity-measurements or real velocities is very obscure.

A cross-wind undoubtedly causes abnormal Deviation of Floats not much submerged, (especially of Surface-Floats;) but from the system of recording only those Floats (Ch. IV, 31) which ran in "fair course", this *does not affect the recorded Results,* the only practical effect being then the increased time required to observe a given number of Floats in "fair course".

Neither does it seem to affect the Mean Velocity-measurements obtained as the quotient $\mathbf{V} = \mathbf{D} \div \mathbf{A}$: for the abnormal water-level

registered at the Gauge in consequence of high cross-wind affects all the Depths (H) which enter into the computed Discharge and Area (D, A) alike, so that any Error due to this is *sensibly eliminated* (*see* also Art. 5) in forming the quotient.

In fact the only Error likely to occur seems to be *of another kind*, viz., in recording the Results, the (normal) Mean Velocity-measurement (V) would be *attributed to an abnormal Gauge-Reading*: thus finally—

"In velocity-work in a high cross-wind, the Mean Velocity-measurement (obtained as above) is not sensibly affected, but is *attributed* to an abnormal Gauge-Reading", ... (24).

17. Variation of *c*.—A glance down Col. 8 of Abstr. Tab. 14—18 will show that—

"The ratio *c* increases in general with decrease of hyd. mean depth", ...(25), though there are a good many cases in which this Rule seems masked or even reversed, (*see* especially Soláni Right Aqueduct [with Left Aqueduct closed], Tab. 14; and Jaolí Site, Tab. 18.)

The evidence in favor of the Rule is very strong, as it includes those cases Tab. 15—17 in which the Range of depth is very great. The Rule might in fact be anticipated as a consequence of the Property of the Mean Velocity Curves (Ch. XVII, 12, vi) of "Increase of flatness with decrease of depth"; for with increase of flatness, it is clear that the Central Mean Velocity (U_0) decreases relatively to the Mean Velocity (V), and their ratio ($c = V \div U_0$) therefore increases.

The exceptions show that the ratio *c* depends also on some of the other External Conditions. The Experiments do not distinctly show what this may be, but from the explanation above that "Increase of flatness of the Mean Velocity-Curve involves increase of the ratio", together with the known property (*ib.*) that "Curves of low velocity are flatter than those of high velocity", it would appear probable that—

"The ratio *c* should increase with decrease of velocity", ...(26), and therefore also (since velocity increases and decreases with surface-slope) probably with decrease of surface-slope.

The Experiments confirm this to some extent only; on looking down Col. 8 of Abstr. Tab. 14—18, it will be seen that any unusually low velocity (occurring between higher ones) is frequently accompanied by somewhat higher values of *c* than its neighbors (*see* especially Ser. 133, 123, Tab. 14, 15). The whole "Range" of the ratio *c* is however so small at any one Site, that the effect is obscure, and it seems not worth while to attempt to formulate it.

For the same reason also, no variation is traceable due to the nature of the banks and bed of Site.

18. Variation of c— The variation of this ratio is still more obscure than that of *c*. This was to be expected, seeing that the Central Surface-Velocity on which it depends is much more liable to be affected by wind than the velocities on which *c* depends.

At the Kamhera Site alone is there any distinct change of the ratio proceeding *regularly* with change of depth; and this regularity (increase of the ratio with de-

crease of depth) is only existent among "Average values" of c, (Pl. XLVIII,) and disappears altogether in the curve of details of c, Pl. XLIX. From what precedes (Art. 16a, b) it appears that much of this irregularity is due to wind.

Again, at the Soláni Embankment Main Site (Pl. XLVII) there is on the whole a distinct small increase of the ratio c with decrease of depth; masked, however, by frequent great irregularities.

No connexion is traceable at the other Sites. This want of connexion, together with the irregularity noticed, show clearly that other Elements have more influence on this ratio than the mere depth has. What these may be the Experiments do not distinctly show. The extreme Range at any one Site (only ·21, *see* Abstr. Tab. 32) is so small as to make such investigation very uncertain, especially when such an element as the Wind is one of the efficient causes of change.

19. Variation of C, (Pl. XLV—XLIX).—The Diagrams show clearly that as a general Rule—

"The ratio C decreases generally with decrease of hyd. mean depth",..........(27), [*see* Soláni Sites, Pl. XLV—XLVII. This agrees with the results of previous Experiments. (Messrs. Bazin's and Kutter's formulæ for C both make C decrease with decrease of R, (*see* Art. 21, 23). At all the other Sites this Rule seems either masked or even reversed: but these exceptions are neither numerous nor strongly marked].

The evidence in favor of the general decrease of C with R includes those cases (Pl. XLVI, XLVII) in which the Range of R is very great. The exceptions show that the ratio C depends not on R alone (as in the Bazin formula, Art. 21), but on some of the other External Conditions.

19a. Dependence of C on S.—It was thought by Messrs. Kutter and Ganguillet that C depended on S as well as on R, and this dependence is expressed in their formula (Art. 23).

This may be illustrated from these Experiments by comparing the Results (values of C, S) for same Site, or for different Sites:—

1°. *At same Site.* This is easily done by comparing the Curves of C, S in the Diagrams.

Soláni Right Aqueduct, (Pl. XLVI). The larger saliences and depressions of the curves of C, F_1, S are so generally concurrent as to make their mutual connexion seem clear. The only marked exceptions are the two Ser. 119 ($R = 5{\cdot}41$) and 124: these are of little importance, as they contain only one Set each, and were done when the water was *very unsteady* (rose ·25 in 119, fell ·22 in 124, *see* Tab. LX, LXI).

Other Sites, Pl. XLV, XLVII—XLIX). There is *no such marked concurrence* of the Curves of C, S at the other Sites. There is at some of them a marked concurrence of part of the Curve of C with *some one* of the Curves F_1, F_2, F_3, but it does not seem worth following in detail.

2°. *At different Sites.* The Twin Soláni Aqueducts are so nearly alike in all respects as to be very favorable for this comparison. The Table below shows the Results paired together of Series with nearly same value of R at each Site:—

Pair.	1st.		2nd.		3rd.		4th.		5th.		Reference.
Aqueduct.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	
Series,	101	108	103	110	105	112	106	113·115	107	116	Abstract Table 20.
Sets,	3	10	4	10	2	3	3	6	4	5	
R, ...	7·94	7·96	7·64	7·64	7·21	7·12	6·81	6·76	6·43	6·40	
S, ...	189	190	207	193	222	204	206	205	225	207	
C, ...	1·048	1·045	·973	1·004	·926	·999	·933	1·001	·909	·989	

These pairs agree in showing that with nearly equal values of R at similar Sites C increases with decrease of S.

The Belra and Kamhera Sites are also favorable for testing this in another way; thus *see* Tab. 22—

Belra Site. Range of R, 9·02—7·60; of S, 208—191; of C, ?·771—·747.

Kamhera Site. „ „ 4·84—4·07; „ 306—291; „ ·779—·757.

It will be seen that in spite of the great differences of R at these two Sites, the values of C are nearly equal. This shows that the effect of a change in R upon the value of C is liable to be entirely masked by a (compensatory) change of one or both of the following, viz., 1°, Surface-Slope; 2°, Nature of Site. Now the two Sites are *fairly alike*, being both earthen channels; the chief differences being (Ch. III, 14, 16) that the Belra Site is nearly thrice the width of the other, and has its banks revetted with masonry. The effect of change of width has hitherto been *supposed* to be sufficiently allowed for by its inclusion in the element R: admitting this, the Surface-Slope seems quite as efficient as R in affecting C.

Reviewing the whole evidence together, it seems most probable that—

"C varies with S, in a somewhat complex manner; increasing and decreasing with increase of S under different circumstances",.................................(28).

19b. Dependence of C on nature of Site.—These Experiments do not furnish much good direct evidence of the effect of the nature of Site upon the value of C.

To test this *fairly*, the comparison should be made only between values of C in Series with *nearly the same values of* R and *also of* S, (and moreover with the Surface-Slope measurement *made in the same way, i. e.*, on one bank or on both banks, *in both cases*,) and possibly also with same Wind, at each Site. But the number of such instances available is very few. The following is an Abstract thereof:—

Sites.	Similar.		Unlike.		Unlike.		Reference.
Series,	101	108	154	192	156	193	
Sets,	3	10	3	6	1	3	
R, ...	7·94	7·96	8·69	8·65	8·25	8·34	Abstract Tables 20—22
S, ...	189	190	229	231	228	227	
C, ...	1·048	1·045	·832	·893	·760	·887	

These Results agree in showing that, with nearly equal values of R and of S (measured in same way), the values of C are nearly equal at similar Sites, and (sometimes very) unequal at dissimilar Sites, so that it seems—the number of data being too few to generalize certainly—that—

"The value of C depends probably on the nature of the banks and bed as well as on R and S", ...(29).

This agrees with previous Experiments. In Bazin's and Kutter's Formulæ allowance is made for this by varying certain of the so-called "constants" with the nature of the bed, (Art. 21, 23.)

20. Mean Velocity Formulæ.—The subject of approximation to Mean (Sectional) Velocity (V) by formulæ will now be taken up from Art. 8, 9, and discussed in the remaining Articles (20—29) of this Chapter.

20a. FORMULÆ INVOLVING S.—Of formulæ for connecting V with S, not of the Chézy type ($V = C . \sqrt{RS}$, Art. 9), the following *old* ones are discussed, and compared (numerically) in the Mississippi Report (pp. 208—220, 316—319). The names of the proposers, and the approximate dates (pp. 187—197, *op. cit.*) are—

Dubuat (1786), Girard (1803), DeProny (1804), Young (1808),
Dupuit (1848), St. Venant (1851), Ellet (1851).

It will suffice to say that they *are all pretty complex* in form, and *are all rejected* (pp. 319, 322, *ib.*) from the evidence of 30 numerical comparisons, as *not of general applicability.*

Also the following *newer* ones are discussed in Kutter's "New Formula", (Jackson's Translation, Art. 15 and 25):—

Bornemann's, $V = \frac{1}{\gamma} . R^{\frac{4}{3}} . S^{\frac{4}{3}}$, ...(30),

Hagen's, $V = \mu . \sqrt{R} . \sqrt[6]{S}$, ($\mu = 2{\cdot}425$ for metric measures), ... (31),

Gauckler's, $V = a^2 . \sqrt[6]{R} . \sqrt{RS}$, when $S > {\cdot}0007$, ...(32a),

$= \beta^4 . R^{\frac{4}{3}} . S$, when $S < {\cdot}0007$, ... (32b),

wherein γ, a, β are "co-efficients of rugosity" depending on the nature of bed and banks.

As these are also *rejected* by Herr Kutter after extended numerical trial, as *not of general applicability*, they need not be further noticed.

20b. FORMULÆ NOW DISCUSSED.—Only two formulæ (of the Chézy type) have been thought worth extended discussion here—

Bazin's,* $V = C_b \times 100 \sqrt{RS}$, or $C_b . w$, (Art. 21), ... (33).

Kutter's, $V = C_k \times 100 \sqrt{RS}$, or C_k, w, (Art. 23—23c), ...(34).

It obviously suffices to discuss and compare the numerical values of the co-efficient with the experimental value (C) of the same: special symbols (C_b, C_k) are therefore assigned to them.

Two other modern formulæ—not of the Chézy type—are also discussed below (viz., the Mississippi Experiments "New Formula", (Art.

24), and Mr. Gordon's "New Formula", (Art. 25,) chiefly in consequence of the importance of the Experiments on which they are based.

[A formula for Cubic Discharge (Canon Moseley's) was discussed in Ch. XIX.]

The only formula (known to the Author) for connecting V, v_0 is that of Mr. Bazin. This also will be discussed below, (Art. 22—22c.)

21. Bazin's Co-efft. (C_b).—From his own numerous Experiments in small artificial channels, Mr. Bazin considered it to be proved* that C depends chiefly on R, and increases with R in a manner nearly represented by the formula,—

$$C_b = \left(a + \frac{\beta}{R}\right)^{-\frac{1}{2}}, \text{ or } \frac{1}{C_b} = \sqrt{a + \frac{\beta}{R}} \quad \ldots\ldots\ldots\ldots (35),$$

where a, β are numerical co-efficients supposed to depend chiefly on the state of the bed, and *to a small extent only* (not expressed in the formula) *on the Slope*, and therefore nearly constant for each Site. Mr. Bazin gave the numerical values of a, β for only† four cases, to which a fifth (No. V below) has since been added ‡ by Herr Kutter. Reduced for use with English feet § ‡ these are as shown below—

Class.	Bed and Banks.		Values of Constants.	
	Condition.	Examples.	a.	β.
I	Very Smooth,	Planed Timber, Fine Plaster, ..	·457	·045
II	Smooth, ..	Rough Planks, Cut Stone, Brick, ..	·566	·18
III	Fair,	Rubble Masonry, ..	·731	·6
IV	Rough, ..	Earth, ..	·853	3·5
V	Very Rough,	Detritus, ..	1·20	7·0

[The advantage of the introduction of the factor 100 into Chézy's formula will now be apparent. When this factor is not used (*e.g.*, in Bazin's and Kutter's Works) the values of a, β are all inconveniently small quantities, *e.g.*, ·0000045 instead of ·045].

The value of this co-efficient (C_b) has been taken out *separately for every* Series in Tab. 20—22, and is shown in Col. 5 alongside of the Experimental value (C) for comparison. It will be seen that *the agreement is often very poor*, (see especially the values for the Soláni Embankment Main Site at low water, Tab. 21; and for the Belra Site, Tab. 22.)

A portion of this disagreement is due to the mode of taking out the values of C_b. For this purpose Mr. Bazin's assignment of numerical values of a, β in his four Classes has been accepted, and the values of C_b have been taken out by interpolation from printed‡ § Tables *for that Class which agreed best with the Experimental values.*

A closer agreement might obviously have been obtained by accepting only the *form*

* Bazin Expts., p. 125, *et seq.* † *ib.*, p. 130, *et seq.*

‡ Kutter's "New Formula for Mean Velocity", Jackson's Translation, Art. 8, 9, 10.

§ Professl. Papers on Ind. Engng., 1868, Vol. V, pp. 283, 294.

of the formula, and calculating special values of α, β for each Site to suit the Experimental values. A little examination of the Results given will, however, show—from the mode of variation of the discrepancy (which increases greatly at low depths)—that no such adjustment of the constants (α, β) would be nearly sufficient to produce a good agreement, and that in fact,—

"The form of the Bazin formula for C is defective",..............................(36).

There seems in fact little doubt (Art. 19a) that the Expression for C should involve S, and that to its absence the Discrepancies are in great part due.

[These Conclusions are the same as arrived at in Kutter's Work, Art. 6, 7].

22. Bazin's Co-efft. (c_b).—The following Expression was proposed* by Mr. Bazin from his own small scale Experiments for the relation between the Mean and Maximum Velocity ($\mathbf{V}$, V) through a cross-section:—

$$V - \mathbf{V} = K\sqrt{RS}, \text{..} (37),$$

where K is a numerical co-efficient, which appeared to be nearly constant* so long as $RS \div \mathbf{V}^2$ not $>$ ·001 in metrical measures (or not $>$ ·000305 for British feet; or, in other words, so long as C_b not $<$ ·57 also for feet), which, as he remarks, covers most practical cases. Within this limit the constant value is proposed*—

$$K = 14{\cdot}1 \text{ in metric measures,} = 25{\cdot}34 \text{ for British feet,} \text{..............} (37a).$$

[The justification of making K constant appears to the Author very doubtful. The experimental values in Mr. Bazin's own Experiments vary from 22·4 to 9·9 in the *whole* number of 61 cases given, and from 17·0 to 10·7 even in the 43 selected cases in which $RS \div \mathbf{V}^2$ not $>$ ·001, *see* pp. 155, 156, *op. cit.*]

With the help of the fundamental equation $\mathbf{V} = C \times 100\sqrt{RS}$, the expression $\sqrt{RS}$ may be eliminated, so as to exhibit the ratio of $\mathbf{V}$, V,

thus, $$V - \mathbf{V} = \frac{K}{100C} \cdot \mathbf{V}$$

whence, $$\mathbf{V} = \frac{100C}{100C + 25{\cdot}34} \cdot V = c_b \cdot V, \text{..............................} (38),$$

where $$c_b = \frac{100C}{100C + 25{\cdot}34}, \text{..} (39).$$

It should be carefully noted that this co-efficient c_b is the value of the ratio $\mathbf{V} \div V$, and is therefore strictly available† only for computing Mean Velocity ($\mathbf{V}$) *from Maximum Velocity* (V) and *vice versâ*. The *practical* utility of such a Result is *obviously very small*, inasmuch as the *real maximum velocity* does not admit of any ready direct measurement. A result of much greater *practical* use would have been the determination of the value of the ratio $c = \mathbf{V} \div v_0$ for computing Mean Velocity ($\mathbf{V}$) from Central Surface-Velocity (v_0).

[It is distinctly stated in Mr. Bazin's Work (Part III, Chap. I, *passim*) that the ratio *sought* was that between the Mean and Maximum Velocity. The latter velocity was determined in *three* different ways, (*op. cit.*, pp. 145, 146)—

1°, by surface velocity-measurement with surface-floats.

2°, by velocity-measurement at 2 centimètres (= ¾ inch) depth with Pitot's Tube.

3°, by *selection of the greatest* velocity among velocity-measurements made at many points all over the cross-section with a Pitot's Tube.

But the measurements No. 2° seem to have been used only as checks on No. 1°, and

* Bazin Expts., p. 157.

† Observe that $c_b = \mathbf{V} \div V$, whilst $c = \mathbf{V} \div v_0$.

the *Float-velocities seem also to have been rejected* whenever less than the measurements No. 8° (*see* pp. 152, 153), so that *the final Results depend chiefly on the measurement No.* 8°, *i.e., on maximum* (not on surface) *velocities*].

22a. Practical application.—It has been the practice (in India) to employ the co-efficient c_b as if it were the same as c, *i.e.*, for computing Mean Velocity (V) from Central Surface-Velocity (v_0), for want of course of any better approximation to the proper value of c. This usage is in fact *recommended* in the French Academy of Sciences Report* on the Bazin Experts. as giving "sufficient accuracy for practical purposes", and the weight of their opinion has no doubt led to general use in this way.

And, with this usage, it is clear that—since the central surface-velocity is usually *less than the maximum* velocity—

"Mean Velocity-measurements, and therefore also Cubic Discharge-measurements, obtained by applying Bazin's co-efficient c_b to central surface velocity-measurements (v_0) are usually under-estimated",..................................(40).

The only justification for this usage in practice is in fact the sufficiency of the approximation. From the general *considerable excess* of the Cubic Discharge-Measurements of these Experiments over those of the official Canal Tables, (Ch. XIX, 20f,) and from what follows (Art. 22b, c), the *approximation seems insufficient.*

22b. Comparison of c_b, c.—The expression (39) gives the value of c_b in terms of the other co-efficient C, so that Tables of c_b may be prepared from existing Tables of C (such as those of C_b, C_k, &c). The authorized (*i.e.*, Bazin's) values are of course those prepared† from Tables of C_b, (the relation (39) itself being in fact due to numerical comparisons of Mr. Bazin's Experiments).

But, inasmuch as the agreement of the co-efficient C_b with the experimental values of C of this Work has been shown (Art. 21) to be very poor, the agreement between the "authorized" ratio c_b (*i. e.*, computed from C_b) with the experimental values of c of this Work could not be expected to be good. There is in fact one *fundamental* difference, viz., (*see* any Tables† of Bazin's ratio, and also Art. 22c).

"Bazin's ratio c_b increases with increase of R, whereas the experimental value c shows no sign of this".

The following Table shows the values of the two ratios, viz., the experimental c and Bazin's c_b for two cases for each Site, viz., the two Series of highest and lowest water-level.

Site.	Bazin's Class.	Table.	Series.	R	c	c_b	Percentum Discrepancy	Series.	R	c	c_b	Percentum Discrepancy
Solání Right Aqueduct,	II	20	108	7·97	·884	·84	−4·6	125	1·98	·880	·82	−6·8
„ Embankt. Main Site,	III	21	151	9·35	·881	·82	−6·8	180	2·24	·965	·79	−18·6
Fifteenth Mile, ...	IV	22	191	9·48	·847	·78	−8·2	195	7·15	·834	·78	−6·0
Belra, ...	IV	22	201	9·02	·859	·78	−9·3	206	7·59	·862	·78	−9·3
Jaolí, ...	IV	22	211	7·82	·861	·78	−9·3	217	6·33	·850	·77	−9·4
Kamhera, ...	IV	22	221	4·84	·842	·76	−9·5	225	4·07	·875	·75	−14·8
Distributaries, ...	IV	LXX	231	2·79	·853	·73	−14·1	236	1·16	·832	·66	−20·5

* Report, p. xxiii, or transl. in Profl. Papers on Ind. Engng., 1868, Vol. V, p. 287.

† Bazin Experts., p. 328. Profl. Papers on Ind. Engng., 1868, Vol. V, p. 295.

The Discrepancies are all in defect (as was to be expected, *v. supra*), and are moreover so large (from 6 to 21 per cent.) in the Earthen Channels, that it may fairly be said that—

"The under-estimation of mean velocity from the use of Bazin's ratio c is so great in case of earthen channels as to render it of little practical use therein",..........(41).

22c. FURTHER COMPARISON OF c_b, c.—As, however, this expression (39) is the only general one—as far as the Author knows—before the public for reduction of Central Surface-velocity to Mean Velocity, it seemed worth while giving it a further extended trial as a *fundamental relation* between values of these ratios *derived from the same source, e. g.*, between the experimental values c, C. Accordingly the values of the c_b have been computed from the formula $c_b = 100\,C \div (100\,C + 25{\cdot}34)$ with the *experimental value* of C for every Series of the Abstr. Comp. Tab. **20—22**, and are shown in Col. **4** thereof alongside of the experimental value (c).

It will be seen at once that—

1°. The values of c_b are (with trifling exceptions) *all* less than those of c, and commonly more than 10 per cent. short of them.

2°. The values of c_b decrease in general with decrease of depth, whereas no such general decrease is observable in c.

Discrepancy 1° could be to a great extent cured by decreasing somewhat the value of the (so-called) constant K, (the value 25·34 may be supposed that suited to the particular value C_b of C,) but the Discrepancy 2° shows that—

"Bazin's relation $c_b = 100\,C \div (100\,C + 25{\cdot}34)$ between c_b, C is fundamentally incorrect as a relation between c, C, (where $c = V \div v_0$)",................(42).

The incorrectness may be due either to the acceptance of v_0 for V, or to the assignment of a *constant* value to K : the data do not show which.

23. Kutter's Co-efft. (C_k).—This formula is based on an extended examination of modern Experiments on Open Channels previous to 1870. From the discussion in Kutter's Work*, it is clear that the co-efficient (C) depends on S as well as on R. The formula with the constants reduced† for use with English feet is, in the notation of this Work—

$$C_k = \left(m + \frac{1}{f}\right) \div \left(55{\cdot}22 + \frac{100mf}{\sqrt{R}}\right), \text{ where } m = 23 + \frac{\cdot 00155}{S}, \ldots\ldots\ldots(43).$$

Here f is a numerical co-efficient depending on the state of the bed, which may be styled the "co-efficient of rugosity", which varies as far as yet known‡ from ·009 to ·035.

This formula is certainly pretty complex, and computation from it laborious. It seems, however, to be perhaps the best empirical formula yet proposed for computing the Mean Velocity *directly* from Slope-measurements, (*i. e.*, without Velocity-measurements.)

[Short Tables of the value of this co-efficient, (reduced from Kutter's Tables for use with British feet,) are given in Jackson's Hydraulic Manual, pp. lxxi to lxxx; and (reprinted from Jackson's Work) in the Roorkee Treatise on Civil Engineering, Vol. II., 3rd Ed., pp. i to v of Appendix. Extended Tables of the same have also

* Kutter's "New Formula for Mean Velocity", Jackson's Translation, Art. 7.
† *ib.*, Art. 31. ‡ *ib*, Art. 28.

been published by the same author under the Title of "Canal and Culvert Tables", (London, 1878,) which give the values of C_k by inspection for eight values of f, viz.—

f = ·010, ·013, ·017, ·020, ·0225, ·025, ·0275, ·030].

23a. COMPARISON OF C_k, C.—The value of this co-efficient (C_k) has been taken out *separately for every* SERIES in Abstr. Tab. 20—22, and is shown in Col. 5 alongside of the experimental value (C) for comparison. It will be seen that the agreement is *on the whole* pretty close, though single instances of rather large discrepancy occur.

This is partly due to the mode of taking out the values of C_k, viz., by interpolation from Jackson's large Work (Canal and Culvert Tables), the Tables in which comprise only the eight values of the rugosity-co-efficient (f) above noted. Those Tables were selected which gave values of C_k *closest to the experimental values* (C), (without reference to the classification given by the author). The Tables selected were those computed for the values of f shown below :—

SITE.		JACKSON'S TABLES OF KUTTER'S CO-EFFICIENT (C_k).	
		Values of f.	Classification. [Jackson's Tables, pp. 79—86.]
Solani	Twin Aqueducts, ..	·020	Rubble in cement in bad condition.
	R. Aqueduct (L. Aq. closed),	·017	Brickwork in Aqueducts in bad order.
	Embankment Main Site, High water,	·025	Earthen Channels, in average order.
	Embankment Main Site, Low water, ..	·030	" " in bad order.
Fifteenth Mile	Old Site, ..	·025	" " in average order.
	New Site, ..	·0225	" " above average order.
Belra, ..		·030	" " in bad order.
Jaolí & Kamhera, ..		·025	" " in average order.

It will be seen that the Classification by no means closely agrees with the actual condition of the Sites (Ch. III) ; thus the mode of selection gives on the whole a much closer agreement than could have been obtained *à priori*. On the other hand a still closer agreement might have been got by determining special values of the rugosity-co-efficient for each Site, and recomputing all the values therewith. The practical advantage to be gained did not, however, seem to warrant the great labor that would have been incurred.

23b. DISCREPANCY OF C_k, C.—The following is an analysis of the Discrepancies showing the number of Series in which the Discrepancy (C_k − C) exceeds 10 per cent., 7½ per cent., 5 per cent., and 3 per cent. (computed on the experimental value C), out of the Total of 83 Series available in the Abstract Tables 20—22).

Over 10 per cent. 13 ; Over 7½ per cent. 5 ; Over 5 per cent. 15 ; Over 3 per cent. 17 ; Under 3 per cent. 33 ; } Total 83.

An Abstract of certain data of the 13 cases of High Discrepancy (over 10 per cent.) is given in the Table following (taken from Comp. Tab. LVIII—LXIV and 20—21) from which it will be seen that in many of the cases the Surface-Slope measurement (S), and therefore also the experimental value of C depending on it are not nearly so well determined as the rest.

SOLANI SITES.	Series.	Sets.	DATA. R	DATA. S	RESULTS. C	RESULTS. C_k	Discrepancy, (per cent. of C).	Causes of uncertainty in Slope-measurement (S), and in deduced Co-efficient, (C).
Left Aqueduct,	105	2	7·21	222	·926	1·032	+ 11·4	Water fell ·12 in one Set.
" "	107	4	6·43	225	·909	1·014	+ 11·6	High wind on 2 days.
Right Aqueduct,	138	1	2·61	145	1·303	1·020	− 21·7	Water rose ·13.
(L. Aqued. closed),	139	2	2·53	151	1·132	1·014	− 10·4	Water fell ·11.
High water.	156	1	8·25	223	·760	·861	+ 13·3	?
High water.	163	6	6·19	171	·943	·824	− 12·6	?
Embankment Main Site, Low water.	170	1	4·76	165	·550	·657	+ 19·5	Water fell ·07.
	173	5	3·87	088	·734	·631	− 14·0	?
	174	1	4·34	125	·575	·647	+ 12·5	Water fell ·28.
	177	3	3·62	193	·558	·618	+ 10·8	Water rose ·10 in one Set.
	179	1	3·11	180	·511	·596	+ 16·6	?
	180	2	2·25	148	·478	·547	+ 14·4	Water rose ·08 in one Set.
	181	1	1·73	090	·352	·501	+ 42·3	Water rose ·08, High wind.

Thus,—

1°. Most (9) of the 13 Series comprise *only one or two Sets*, so that the Slope-measurements are not good Average values.

2°. The water was *unsteady* in 8 Series.

3°. The wind was *unfavorable* in 2 Series.

4°. Most (9) of the 13 cases occur *at low water*, a stage at which the water-level determinations (on which S depends) were not so accurate at the Sites in question, as at higher levels, (Ch. V, 7b).

5°. In all the cases the Slope-measurement was made *only on one Bank*.

6°. In most (10) of these Cases the Surface-Slope is *unusually low* (< ·000200).

This last Result (6°) points to the Conclusion that most probably—

"In Kutter's Co-efficient the Surface-Slope (S) has not been given due importance",..(44).

23c. Kutter's Co-efft., CONCLUSIONS.—On the whole it may be said that Kutter's Co-efficient stands the test of comparison with these Experiments fairly well: combining this evidence with the very varied evidence in Kutter's Work, it may be fairly said that—

"Kutter's Co-efficient is one of pretty general applicability",....................(45).

It will of course be understood from the mode of its derivation (*see* Kutter's Work) that it is a *purely empirical one.*

Probable Error. From the analysis of Discrepancies above given, and remembering that those over 10 per cent. are probably in part due to imperfect determination of the Experimental Co-efficient (C), it would seem that—

"When the Surface Slope-measurement is a *good average*, Kutter's Co-efficient will give Results whose Error will probably seldom exceed 7½ per cent. in Large Canals",..(46).

But for this accuracy, it seems essential that—

"The Slope-measurement should be done on both banks, always in calm air, and only when the Canal is in train, and should be repeated several times",.........(47).

And further, for this accuracy, it is essential that the "rugosity-co-efficient" (f) be properly known for the Site *in the first instance.* This of course can only be properly determined by direct Experiment, *i.e.*, by determining a few *experimental* values of C from which to determine f, either by direct calculation, or by comparison with published Tables. If the value of f be merely selected *à priori* by comparing the known state of the Channel with the published Classification, so close an approximation will be a mere chance.

24. Mississippi Expts. Formula, (Miss. Report, p. 312).—Upon a certain theoretical investigation the following formulæ are proposed in the Mississippi Report for calculating Mean Velocity (**V**) from the Surface-Slope and Cross-Section data. With the notation used in this Work, the formulæ are—

For rectangular cross-section, $\mathbf{V} = \left(\sqrt{\beta' + (195\, r \sqrt{S})}^{\frac{1}{2}} - \sqrt{\beta'}\right)^2,$

where $\beta' = \dfrac{\cdot 010816}{\sqrt{R + 1\cdot 5}},$ ....(48),

For earthen channels, $\mathbf{V} = \left(\sqrt{\beta'' + (225\, r \sqrt{S})}^{\frac{1}{2}} - \sqrt{\beta''}\right)^2,$

where $\beta'' = \dfrac{\cdot 013689}{\sqrt{R + 1\cdot 5}},$ ....(49),

in which, $r = \dfrac{A}{b + B}$

In these expressions β', β'', $\sqrt{\beta'}$, $\sqrt{\beta''}$ are all small quantities which cannot exceed the following values (corresponding to $R = 0$) :—

$\beta' = \cdot 00879$, $\beta'' = \cdot 0111$, $\sqrt{\beta'} = \cdot 094$, $\sqrt{\beta''} = \cdot 105$,......(50),

so that they do not much affect any but low velocities. Thus at high velocities these expressions approximate to—

$\mathbf{V} = 14\sqrt{r}\,.\,\sqrt[4]{S}$, and $\mathbf{V} = 15\sqrt{r}\,.\,\sqrt[4]{S}$,....................(51),

so that they differ markedly from the older expression $\mathbf{V} = C \times 100\sqrt{RS}$ in two points—

1°, in the change from $R = A \div B$ to $r = A \div (b + B)$.

2°, in the change from $\sqrt{S}$ to $\sqrt[4]{S}$.

Below are given the data for, and results of, application of this formula to a few selected cases of the present Experiments: the cases have been chosen so as to present instances of high and low water-level, and also of high and low Surface-Slope as far as available at each Site. In the Table the Mean Velocity value of the formula is denoted by $\mathbf{V}'$, and the experimental value by $\mathbf{V}$.

It will be seen at once that the values ($\mathbf{V}'$) from the formula are in many cases *no sort of approximation* to the experimental values. The Conclusion seems inevitable—

"The Mississippi Experiments' Formula is useless as a *general* formula for Mean Velocity",..(52).

The same Conclusion is arrived at in Kutter's Work: from the investigations therein (Art. 5, 6, *op. cit.*) it seems probable that this formula gives as a rule fair approximations *only in cases of very low slope.*

SITE.	Series.	DATA.						RESULTS [of formula]		By Expt.
		R	b	B	A	r	S	β' or β''	V'	V
Soláni Left Aqueduct, ..	101	7·94	82.2	105·7	839·5	4·47	189	·00352	3·25	4·06
	107	6·43	85·0	99·3	638·7	3·47	225	·00384	2·97	3·46
Soláni Right Aqueduct,	108	7·96	82·1	105·8	841·8	4·48	190	·00351	3·26	4·06
	122	4·02	85·0	92·5	371·9	2·09	278	·00460	2·40	3·26
	127	·74	84·4	85·2	63·2	·37	113	·00721	·73	·60
Soláni Right Aqueduct (with L. Aqueduct closed),	131	4·17	85·0	92·9	387·1	2·17	025	·00454	1·30	1·24
	132	3·68	85·0	91·7	338·1	1·91	473	·00474	2·62	4·83
Soláni Embankment Main Site, ..	151	9·34	170·0	191·1	1784·9	4·94	227	·00416	3·84	4·02
	159	7·66	162·6	179·2	1373·4	4·02	243	·00452	3·50	3·29
	171	4·76	150·0	158·4	754·1	2·45	038	·00548	1·65	·93
	181	1·73	150·0	151·9	262·9	·87	090	·00761	1·18	·44
Fifteenth Mile Old Site,	192	8·65	174·9	180·0	1557·2	4·39	231	·00429	3·63	3·98
Fifteenth Mile New Site,	196	8·68	185·9	191·7	1664·7	4·40	221	·00429	3·59	4·12
Belra, ..	201	9·02	188·4	196·4	1772·4	4·60	191	·00423	3·54	3·17
	206	7·60	186·8	193·3	1468·8	3·86	200	·00453	3·26	2·92
Jaolí, ..	211	7·82	192·8	200·2	1565·7	3·98	174	·00449	3·20	2·96
	217	6·32	190·9	196·1	1240·2	3·20	140	·00489	2·69	2·63
Kamhera, ..	221	4·84	65·5	69·5	336·0	2·49	295	·00543	2·85	2·86
	225	4·07	64·0	67·1	273·2	2·09	306	·00580	2·62	2·71

25. Gordon's New Formula.—A new formula for Mean Velocity is proposed in the Work* quoted (p. 42) based upon certain theoretical reasoning† which appears to the present Author defective. It is unnecessary to discuss this, as it is admitted (*ib.*) that the formula *does not agree with Experiment,* and that to make it agree with a selection of the Irrawaddi and Bazin Experiments, certain empirical changes have been made in it. The modified formula is—with the notation of this Work,

$$V = \sqrt{\frac{\bar{z}bS}{B} \div \left(\mu + \frac{\nu b}{R^2\sqrt{S}}\right)} \qquad \text{(53)},$$

where $\bar{z}$ = depth of "centre of pressure",

μ, ν are numerical co-efficients to be determined by Experiment.

The empirical changes made are so great that the modified formula is really *purely an empirical one.* The labor involved in the application of this formula is very great from the difficulty of calculating the quantity denoted by $\bar{z}$. It is by no means clear even what is meant by the term *centre of pressure* in the case of water in motion.

[The "centre of pressure" meant is apparently (from its definition on p. 29, *op. cit.*) that of *hydrostatic* pressure upon a plane surface occupying the position of the cross-section].

The uncertainty of application of this formula appears so great that—viewing the great labor of the computation—it was not thought worth while to test it.

26. New empirical trials hopeless.—The great uncertainty of suc-

* "Gordon's New Formula"; For full Title, *see* Ch. I, 21, List II.

† in part the same as set forth in Ch. X, 13a.

cess in formation of these more or less empirical formulæ, the great probability that increased approximation can only be obtained by increased* complexity, and the great labor† attending the numerical verification seem (to the Author) to make further research for an improved *form* of co-efficient almost hopeless from the experimental side, *i.e.*, until some help as to the *proper* functional form can be obtained from Theory. Accordingly *no new formula is now suggested.*

27. Ranges of *c*, c, C.—In the absence of any true formula for these co-efficients, that one would probably be the most practically useful which was the *least variable*, inasmuch as less Error would be likely to result from using it. It is therefore important to compare the extreme "Ranges" (of the experimental values) of these co-efficients.

To do this *quite fairly*, it is obvious that the Ranges should be taken only of values of the co-efficients obtained together. It is unfortunately impossible to do this properly from the printed Tables inasmuch as—

1°. Detailed values are given only for c, C, (Tab. LVIII—LXX).

2°. The mean values given of *c*, c, C (Tab. 14—18 & 20—22) in Series bearing the same Serial No. are frequently derived from a different number of data (*i.e.*, Sets), *e.g.*, in Ser. 108—

c depends on 19 Sets (Tab. 15), c on 7 Sets, and C on 10 Sets, (Tab. 20.)

In comparing the Ranges *c*, c, C from the printed Tables, it is necessary therefore to bear in mind at same time the difference of the number of data available in each case; because, the greater the number of data, the greater is the probability of variation (from the chance of greater variation of External Conditions). These are brought together in Table below.

Description of Range.	*c* No.	*c* Range.	c No.	c Range.	C No.	C Range.	Reference to Tables.
Range of detailed values in whole Experiments,	581	?	331	·225	376	·951	LVIII—LXX.
Highest Range of detailed values at one Site,...	153	?	71	·215	92	·721	
Range of Serial values in whole Experiments,...	100	·237	76	·224	83	·951	14—18, 20—22 & LXX.
Highest Range of Serial values at one Site, ...	31	·216	26	·205	27	·591	

It will be seen that—

"The variation of C is much greater than of *c* or c", ... (54*a*).

"The variation of *c* is only slightly greater than that of c", ... (54*b*).

Further, from Tab. 20—22 it appears that—

"Within each Series the Range of C usually (though with some striking exceptions) greatly exceeds that of c", ...(54*c*).

* Witness the successive increases of complexity from Chezy's to Bazin's and Kutter's Formulæ.

† Kutter's Discussion of the formula proposed by himself occupies an 8vo. volume of 95 pages, (without showing the numerical details of the steps on which its verification is based).

27a. USE OF c, c BETTER THAN C.—It would seem that in the present state of hydraulic knowledge, *i.e.*, chiefly in consequence of the smaller Total Ranges—

"The Co-efficients c, c are more likely to give fair approximation to Mean Velocity than the Co-efficient C", ...(55),

or in other words—

"A close approximation to Mean Velocity is more likely to be obtained by use of formulæ depending on Velocity-measurement than on Surface Slope-measurement", ...(55*a*).

[Compare with this Ch. XIV, 12, wherein a similar Result was obtained for the case of approximation to Mean Velocity past a Vertical].

27b. USE OF c BETTER THAN c.—Again, it will be seen from Art. 11, that the effects of Unsteady Motion have been very imperfectly eliminated from the value of U_0, and therefore also from the experimental value of c. Had each velocity U_0 been 48 times observed (like v_0), it seems probable that the Range of c now shown would have been reduced. On the other hand the Range of c would be increased, if the number of data (Sets) available for it were increased to the number* available for c. Thus it seems that in a strictly fair comparison—

"The Range of c would probably exceed that of c", ...(56).

This seems likely *à priori* inasmuch as the central surface-velocity on which c depends is largely affected by wind, whereas the central mean velocity is probably but slightly affected.

Under these circumstances, it seems that (possibly chiefly on account of the uncertainty caused by wind)—

"Central Mean Velocity-measurement is to be preferred to Central Surface Velocity-measurement for use in approximating to Mean Sectional Velocity",...(57).

This last Result, together with Result (55*a*), are conceived to be the most important practical Results of this Chapter.

28. Velocity-and Slope-Results compared.—The Conclusion (55*a*) that a closer approximation to Mean Velocity is obtainable by actual Velocity-measurement than by Surface-Slope measurement is worth further consideration. There can be little doubt that the Mean Velocity is in some way conditioned by the Surface-Slope, but the law of connexion is at present *wholly unknown*, the present formulæ being in fact purely empirical, *resting on no rational basis*, and therefore *only of limited and uncertain applicability*. The connexion between the Mean Velocity and any particular velocity is indeed also unknown, but there can be little doubt

* The data (*i.e.*, Sets) available for c are far fewer than for c, *see* Art. 10a and 27.

that the connexion is a *far more intimate and less complex one* than that between Surface-Slope and Velocity. Thus it appears from Ch. X, 15 & XVIII, 2, that the velocities at all parts of a cross-section increase or decrease somewhat similarly with any change of External Conditions, so that it may be said that, given any one velocity, the difficult problem of translation of the various External Conditions into "terms of velocity" is already done, and the *simpler problem left* of finding Mean Velocity from the given velocity. In other words, the deducing Mean Velocity from the External Conditions (one of which is Surface-Slope) is an *indirect* problem, and the connexion is a *physical* one: whereas the deducing Mean Velocity from a given Velocity-measurement is a *direct* problem, and the connexion is possibly *only a geometrical* one (depending solely on the figure of the velocity-surface).

Add to this that Surface-Slope measurement is a matter of great uncertainty and delicacy—

Uncertainty. The proper mode of measurement is very far from properly known; inasmuch as different Slope-Lengths are liable to give different values of the Surface-Slope, (Ch. VII, 5a.)

Delicacy. This results from the smallness of the quantity to be measured, sometimes so extremely small (especially in Large Rivers) as to be less than the probable errors of measurement (Ch. VII, 2b,c).

The fact is that the attempt to compute Mean Velocity from Surface-Slope-measurement is generally a *working from small to large quantities* involving risk of large error. The Surface-Slope could in fact (whenever very small) be *much better* computed from the Mean Velocity* than directly measured.

29. Approximation recommended.—Whenever time and means admit the direct process of Discharge-measurement developed† in Ch. XVII XIX should be adopted. Some simplification for practical use will be considered in Ch. XXII. But when rapid approximation is necessary, Results (55*a*), (57) point to the Conclusion that—

"For rapid approximation to Mean Velocity a good Central Mean Velocity-Measurement is (at present) the most reliable",....................................(58).

As the Discharge-measurement is to depend on a Velocity-measurement *at a single point*, it is of course essential—in consequence of the Unsteady Motion—that this Velocity-measurement should be a good Average one, *i.e.*, the Mean of about 50 trials (Ch. VI, 10). The *proper*

* This suggestion is made in the Révy Expts., p. 122.

† The Field-work is explained in Ch. XVII, 7.

application of this requires of course a *prior knowledge* of the value of the ratio $c = \mathbf{V} \div U_0$; this being at present unknown *à priori*, should (properly speaking) be determined *by direct Experiment for each Site*, thus—

STEP I. A few fundamental Cubic Discharge-measurements (**D**) should be made for the Site by the direct process of Ch. XVII, XIX; the Average Central Mean Velocity-measurements (U_0) should be made at middle of each Discharge-measurement.

[The Central Mean Velocity-measurements should be repeated about the same number of times as the Total number of velocity measurements upon which the Discharge measurement depends, so that both measurements U_0, **V** may be of equal precision, Art. 11].

STEP II. The fundamental Mean Velocity-measurements (**V**) are to be computed from the above by the formula $\mathbf{V} = \mathbf{D} \div A$.

STEP III. These will furnish the "fundamental values" of the ratio c to be computed as the ratio $\mathbf{V} \div U_0$. Others can be obtained by interpolation.

[Without this prior determination of values of c special to the Site, a good approximation could not be expected].

CHAPTER XXI.

DISCHARGE-VERIFICATION.

Preface.—This Chapter contains a comparison of Cubic Discharge-measurements which it is known *à priori* ought to be nearly equal. The approximate equality of these is taken to be a proof of the consistency of the Results. The most important Articles are Art. 1, 2, 3c, 4b,c, 5, 6—6b, 7a, 8, 8g, 9, 9b,c, 9h, 10—12, especially the last three. The Results (Art. 12) are of great practical importance.

1. **Discharge-Verification.**—The importance of the question of Cubic Discharge-measurement is so great in practical Hydraulics, that it is very desirable to obtain if possible some verification, or at any rate some *Test of the degree of accuracy* of the Results, as this would afford a measure of the suitability of the process used.

The only TEST that can be considered as yet a perfect Test, and therefore a complete verification, is the receiving the quantity discharged into large measuring tanks. This is comparatively easy with small channels, but is a practical impossibility with large quantities of water like those here treated of. Some less perfect Test must therefore be sought.

It is clear that any process whatsoever that is worth using should give Results which are *consistent with each other*. The obvious constancy of the Cubic Discharge in the following cases,—

i. At the same Site *under similar External Conditions*,

ii. At successive Sites in the same channel (between which there is no water admitted or drawn off), *under similar External Conditions*,

seems to be a suitable Test of any process of Discharge-measurement. The application of this Test consists merely in the comparison of the numerical Results (Cubic Discharges) obtained *under similar External Conditions*. Exact coincidence could not be expected on account of the unavoidable imperfection of the Results, and especially in consequence of the effect of the Unsteady Motion of the water thereon, Ch. (XIX, 19d). Approximate coincidence is all that can be looked for.

[On account of the great importance of this matter, such Verification as is possible of the consistency of the Results obtained by the process used on this work will be now discussed at length].

2. Test i, Discharges at same Site.—It is obvious that the Discharge-measurements to be compared should have been executed under as nearly as possible similar External Conditions. Three applications will be made of this Test, and will be distinguished briefly as Tests *ia*, *ib*, *ic*, viz.,—

TEST *ia*. Discharge-measurements in same Series, (Art. 3—3d.)

TEST *ib*. Discharge-measurements on same day, (Art. 4—4c.)

TEST *ic*. Simultaneous Discharge-measurements with different Runs, (Art. 5.)

3. Test ia, Results of same Series.—It has been explained that the several SETS of any one Series are pieces of Field-work done at the same Site under as nearly the same External Conditions as the varying state of the Canal permitted.

[The Range of water-level does not exceed ·3 in any one Series: the Range of Surface-Fall is—though usually trifling—unfortunately rather large in a few cases: the Range of Wind too is often large].

It suffices therefore for the application of this TEST to compare the values of the Cubic Discharge *within the same Series* throughout (Col. 7 of) the Detailed Tables XXXIV—LVI.

A rough sort of agreement will be obvious at once: and part of the *apparent* want of agreement is obviously due to the difference of water-level in the different SETS of the same Series; for, as already remarked (Ch. XIX, 17a), the Discharges within each Series are found to decrease *on the whole* with fall of water-level, so that if a correction were applied to reduce all Discharges within a Series to a common water-level, much of the disagreement would be got rid of.

There are, however, numerous cases of discrepancy which cannot be so accounted for, viz:—

1°. Many cases of discrepancy *at same water-level.*

2°. Many cases in which the discrepancy is *too large* to be accounted for by the mere change of water-level.

3°. Many cases in which the discrepancy is even *of opposite kind*, (the higher Discharge-measurement corresponding to the lower water-level.)

In these last three cases it is obvious from the mode of computation of the Discharge-Results that—as explained in Ch. XIX, 17b—the velocity-factor is the *proximate* cause of the apparent discrepancies in the Results; its change (within the same Series) being sufficient to neutralize or even to reverse the effect of change of water-level.

In carrying out this TEST in detail, it is obviously desirable to apply the correction for change of water-level as a preliminary, so that the *source* of discrepancy may be sought in the other External Conditions.

Now it is clear that the actual Discharge-measurements (of the same Series) might be corrected for both change of water-level and change of surface-breadth (so as to reduce all the Results to a common water-level and surface-breadth) by the following simple process:—

$$\left.\begin{array}{c}\text{Corrected Discharge-}\\\text{measurement}\end{array}\right\} = \frac{\text{Actual Discharge-measurement}}{\text{Actual Area}} \times \text{Mean Area of Series},$$

$$= \text{Actual Mean Velocity} \times \text{Mean Area of Series,}\ldots\ldots(1).$$

Thus the Mean Velocity-measurements are proportional to the sought "corrected" quantities, and are therefore *suitable for comparison in place of these latter.*

[They have the advantage also of showing distinctly that the residual discrepancy (after applying the correction for change of level) really lies in the velocity-factor, and the additional (very great) practical convenience of being comparatively small quantities, so that their examination is easy].

3a. Test ia, TABULATION OF RESULTS.—A detailed examination of *every* case of Discrepancy would run to enormous length: it will suffice for the present purpose to examine only the larger Discrepancies. Abstr. Tab. 24 affords the means of selecting these cases: it shows separately *for every Mean Velocity Series* (from Tab. XXXIV—LVI)—

1°, the number of Sets in the Series.

2°, the highest Mean Velocity (V) of the Series.

3°, the Mean Velocity Range, both actual, and per cent. (upon the highest).

The entry of the number of Sets gives a measure of the chance of occurrence of a high Range in the Results, this chance of course increasing *cæteris paribus* with the number of Sets.

The following is an Abstract of the above, showing separately for each Site, the number of Series (containing more than one Set), total number of Sets, maximum percentum Range of Mean Velocity, and number of Series in which this Range exceeds 10 per cent., and exceeds or falls short of 5 per cent.

SITE.	Number of Series.	Number of Sets.	Maximum per cent. Range.	NUMBER OF SERIES of High and Low Range.		
				Range. > 10 °/o	Range. < 10 °/o, > 5 °/o	Range. < 5 °/o
Soláni Left Aqueduct, ...	7	45	7·0	0	2	5
Soláni Right Aqueduct, ...	18	167	19·7	5	4	4
Soláni Right Aqueduct, ... (with Left Aqueduct closed).	3	6	7·5	0	1	2
Soláni Embankment Main Site,	27	149	17·1	9	10	8
Fifteenth Mile Sites, ...	4	14	9·4	0	1	3
Belra, ...	6	53	9·1	0	5	1
Jaolí, ...	7	55	9·5	0	6	1
Kamhera, ...	5	56	8·8	0	4	1
Distributaries, ...	6	14	5·1	0	1	5
Totals, ..	78	559	..	14	34	30

It will be seen at once that in a large number of Series the percentum Range is too small to be worth inquiry, and that it is excessive (over 10 per cent.) in only a small number (14 out of 78).

3b. Test ia, EXCESSIVE DISCREPANCIES.—It is proposed to limit the detailed inquiry to these Series of excessive Range (over 10 per cent.) in the first place at any rate. Abstr. Tab. 25 contains the data for this, being an Abstract of all elements which seem likely to affect the Mean Velocity-measurements in question, viz.—

1°. Gauge-Reading, and Variation of same.

2°. Length of Rod used, (given only for the Soláni Aqueducts).

3°. Surface-Fall in each Sub-Reach (F_1, F_2, F_3), and Surface-Slope (S).

4°. State of the Wind.

The cases to be compared are selected as follows:—

1°. Every "very high" mean velocity (exceeding the lowest of the Series by 10 per cent.) is first entered as the "Argument": also the value of $\frac{9}{10}$ of the same as a guide to the selection of the "very low" mean velocities.

2°. Every "very low" mean velocity not exceeding $\frac{9}{10}$ of the preceding high one, or only the highest and lowest of the same when more than two in number; and in this last case only the highest and lowest values of the several elements above-mentioned (viz., l, F_1, F_{ll}, F_2, S and Wind) are given: also the number of such cases.

The date is given, as a means of identification in the Det. Tab. XXXIV—LVI, except when the cases of low mean velocity to be examined exceed two in number, in which case (to save space) the reference numbers of each Set concerned are given for the same purpose. The number of cases of high (10 per cent. and over) discrepancy in the Table will be seen to be only* 88 in all.

3c. Test ia, DISCUSSION.—On the right hand of the Table is given a note of what appear to be the Probable Causes either of the unusually high velocities or of uncertainty in the Results. It will be seen that—

1°. In every case the Surface-Fall in at least one of the Sub-Reaches (usually the Upper), and sometimes the Surface-Slope are *markedly high with the high velocities.*

2°. In several cases the use of a Short Rod seems to have enhanced the effect.

3°. In several cases Unsteadiness of the water, and perhaps High Wind have produced uncertainty in the Results.

Thus it appears that in all the cases of excessive Discrepancy the variation of External Conditions was such as to *account at any rate in part* for it. The mere collocation of Sets in the same Series is therefore not sufficient to warrant the certainty of close similarity of the External Conditions, so that *close agreement in the Mean Velocities could not be expected.*

3d. Test ia, RESEARCH TEDIOUS.—It might be thought desirable to pursue the inquiry into other cases of high (though not excessive) discrepancy, exceeding say 5 per cent. The number of such cases is, however, so very large, occurring in 48 Series (*see* Table of Art. 3a), and, in great numbers in some of these Series, (*e.g.*, upwards of 64 in Ser. 109), that the work would be unduly extended.

[As a preliminary in this direction, a Table was prepared showing the details of all available elements affecting the *highest* and the *lowest* mean velocity of every one of the above 48 Series. From this it was at once seen that the high velocity was in nearly every instance accompanied (as in the former case) by a Surface-Fall markedly higher (in at least one of the Sub-Reaches) than the Surface-Fall with the lower velocity. In other words, the variation of External Conditions was commonly such as to account *at any rate in part* for the discrepancy.

It seems unnecessary to publish this Table: the above simple statement will probably be accepted as sufficient].

4. Test ib, Results of same day.—To continue this inquiry without

* This is really a *very small* proportion. Every pair of Sets (within the same Series) is in this view a *comparable pair*. Each Series of n Sets yields of course $\frac{1}{2}n(n-1)$ such "comparable pairs". The Total number of these pairs is 2,714 (as may be readily verified from Tab. 24, which shows in one view the number of Sets in every Series herein concerned).

running to undue length, it seems therefore desirable to make a selection of cases in which the constancy of the conditions affecting the velocities can be inferred with greater certainty than in the previous rough selection, simply as being contained within the same Series.

There is fortunately a pretty large number (upwards of 100) cases available of two, three, or more (up to six) Discharge-measurements made *on the same day*, and *generally in close succession.* This selection will probably give in general as close an approach to constancy of such of the External Conditions as depend on the state of Control (excluding therefore Wind) as can be ordinarily obtained.

[This constancy is, however, by no means uniform, as the water was undergoing rapid change of level on some few of the days in question. This will appear below].

As before, it is proposed to examine in detail only the cases of higher discrepancy. Abstr. Tab. 26 affords the means of selecting these: it shows *for every day* on which more than one Discharge-measurement was made—

1°, the Serial No. and Date, as a means of identification.

2°, the Gauge-Reading, and the resulting Mean Velocity for every Discharge-measurement of the day.

3°, the Range of the Mean Velocities of the day.

4°, the per cent. Range estimated on the highest Mean Velocity of the day.

The sign + or − prefixed to the Range indicates that the change of Mean Velocity is *of same kind as* or *of opposite kind to* the change of water-level. No sign is attached when the water-level remained constant.

The following is an Abstract of the above, showing for each Site separately as follows:—

1°, the number of pairs, trios, &c., &c.

2°, the maximum per cent. discrepancy.

3°, the number of cases of discrepancy under and over 3, 5, and 10 per cent. respectively.

SITE.	NUMBER.						Max. per cent. Range.	NUMBER OF GROUPS OF HIGH AND LOW RANGE.			
	Pairs.	Trios.	Fours.	Fives.	Sixes.	Total.		> 10 %	< 10 % > 5 %	< 5 % > 3 %	not > 3 %
Soláni Left Aqueduct, ...	10	1	..	..	..	11	3·8	..	..	1	10
Soláni Right Aqueduct, ..	24	7	6	..	1	38	51·6	1	..	5	32
Soláni Right Aqueduct, [with Left Aqueduct closed.] ..	4	1	..	..	..	5	22·0	2	1	1	1
Soláni Embankment Main Site,	26	4	1	1	..	32	16·7	4	1	6	21
Belra, ...	3	..	..	..	..	3	1·9	..	..	..	3
Jaolí, ...	4	..	..	..	..	4	3·7	..	..	1	3
Kamhera, ...	6	..	..	..	..	6	3·1	..	..	1	5
Distributaries, ...	7	..	..	..	..	7	4·5	..	..	2	5
Totals, ..	84	13	7	1	1	106	..	7	2	17	80

And here a great difference will be at once obvious between these and the previous Results, in the far closer agreement of the present Results (done on the same day). In fact in 80 out of the total of 106 groups, the discrepancy is so trifling (not over 3 per cent.) as not to be worth further inquiry.

[It is true that there are a few cases of excessive Range, (51·6, 22·0, and 21·1 per cent.) But the explanation of these is *obvious*, in that the state of the Canal was undergoing great changes on each of the days in question, as will appear below].

4a. Test ib, DISCREPANCIES OVER 3 PER CENT.—The detailed inquiry is now limited to the 26 groups in which the Discrepancy exceeds 3 per cent. Abstr. Tab. 27 contains the data for this, viz., an Abstract of all the elements which seem likely to affect the Mean Velocity-measurements in question, viz.—

1°. Gauge-Reading (h or H), and Variation of Gauge.

2°. Length of Rod used (l), (only given for the Solání Aqueduct).

3°. Surface-Fall in each Sub-Reach (F_1, F_2, F_3), and Surface-Slope (S).

4°. State of the Wind.

5°. Timekeeper's Initial.

In this Table the several Sets of the same day's work are arranged *in the order in which they were executed in* the Field, for the sake of showing more distinctly whether the water was rising or falling.

[The Table shows *every* case of Discrepancy exceeding 3 per cent. (and no others). This accounts for the apparent disappearance from this Table of the groups of 3, 4, 5, or 6, inasmuch as the Discrepancy seldom reaches 3 per cent. in more than one or two pairs of such groups.]

4b. Test ib, DISCUSSION.—Table 27 is divided into two parts, the first containing all the groups (9) of high and of excessive Discrepancy (over 5, and over 10 per cent. respectively), and the latter all the groups (17) of moderate Discrepancy (over 3, but under 5 per cent.)

The probable Cause of the Discrepancy is shown in the right hand Column.

High Discrepancy, (9 groups, comprising 11 cases each over 5 per cent.). The right hand Column shows at once what appears to be the sufficient Cause of the Discrepancy, viz., that the Canal was "not in train", but undergoing rapid change in each case.

Moderate Discrepancy, (17 groups comprising 18 cases each over 3 per cent.) The Causes of Discrepancy are here obscure, it will be seen that—

1°. In 8 cases the state of the water was too variable* for accurate work.

2°. In 4 cases high wind probably interfered with accurate work.

Most of the 18 Discrepancies are however close to the lower limit, (3 per °/o,) viz.—

6 of 3·1, 3 of 3·3, 2 of 3·6, 1 of 3·7, 2 of 3·8, 1 of 3·9, 1 of 4·0, 1 of 4·5, 1 of 4·8 per cent., thus there are only two cases over 4 per cent., and 9 less than 3½ per cent.

4c. Test ib, LOW DISCREPANCIES, (not over 3 per cent.).—The number of closely agreeing *pairs* of Results may now be shown, moreover, to be much greater than

* This could not of course always be recognized during the progress of the Field-work, the Control occurring at a distance.

would at first sight appear from the above Abstract (of Art 4), which shows a Total of only 106 groups. This number (106) is, however, really the actual *number of days* on each of which two or more Discharge-measurements were made. Now it is clear that each group of 3, 4, 5, or 6 Discharge-measurements (done on same day) yields 3, 6, 10, or 15 *pairs* of comparable values respectively, so that the Total number of comparable pairs is thus raised (*see* Abstract, Art. 4) to a—

Total, $84 + 3 \times 13 + 6 \times 7 + 10 + 15 = 190$ pairs,

out of which there is high discrepancy in 11 pairs (due to known causes), and moderate discrepancy in 18 pairs (partly accounted for), so that there remains a Total of 161 pairs of Results in close agreement.

5. **Test ic, Simultaneous Field-work on different Runs.**—The four Sets of Mean Velocity-measurements made at the Belra Site on 11-2-'79 to test the question of the Length of Run necessary for Float-work (Tab. LXXI), give further valuable evidence on this point.

It was explained (Ch. IV, 28) that every Rod was timed *under four Ropes in succession*, so that the Float-velocities of the same Rod were deduced from 4 different Runs (2 of 25', 1 of 50', 1 of 100'). The Mean Velocity-measurements resulting were (*see* Table)—

Middle 50' Run, 3·00 ; Outer 100' Run, 2·99 ; Discrepancy = ·01, or ·3 per cent. (on max.).
Upper 25' Run, 2·92 ; Lower 25' Run, 3·06 ; Discrepancy = ·14, or 4·6 per cent. (on max.).

The value of this comparison is in that the Field-work of each Set was begun together, carried out together continuously *for every Float-Course*, and finished together, so that these four Sets were subject to *precisely the same External Conditions* (in even minute details), and the Discrepancies of the Results can only be due to—

1°. Errors in timing chiefly affecting the Results in the short (25') Runs,

2°. Unsteady Motion causing different Results in the different Runs.

The comparatively large (4·6 per cent.) Discrepancy in the Results from the two 25 foot Runs, and the close agreement between the Results in the longer Runs confirms the views previously advanced that a Run of 50 feet is sufficient, and of 25 feet too short for general use. The extreme closeness of the agreement between the Results from the longer Runs is probably accidental, but the inference may fairly be drawn that a pretty close agreement might be expected in such work.

6. **Test ii, Discharge-measurements at successive Sites.**—For the proper application of this Test, it is obvious that there should be *no water admitted into, nor withdrawn from, the channel* between the Sites the Discharges through which are to be compared. It is desirable therefore that there should be no inlets into, nor outlets from, the channel between the Sites. It is further desirable that the Sites should not be very far apart, so as to diminish the risk of the occurrence of springs and leaks, and to reduce the amount of evaporation and absorption. Four applications will be made of this Test, and will be distinguished briefly as Tests ii*a*, ii*b*, ii*c*, ii*d*, viz.,—

TEST ii*a*. Non-simultaneous Discharges, (Art. 7, 7a.)

TEST ii*b*. Simultaneous Discharges in same Reach, (Art. 8—8g.)

TEST ii*c*. Simultaneous Discharges in different Reaches, (Art. 9—9h.)

TEST ii*d*. Discharges of same body of water, (Art. 9—9h.)

The successive Sites varied considerably in each application, both in width, in figure of Cross-Section, and in physical state, so that the Test applied is pretty searching.

6a. EFFECT OF FORMULÆ.—As the formulæ used in the Discharge-computation affect the Results (obtained from the same data) sometimes markedly, and therefore differently at Sites of different Cross-section, the Field-process itself would be most fairly tested by comparing Results from successive Sites of similar Cross-Section, *with same Float-Course Spacing*, so that the same formulæ might be used for each.

A far more searching Test, however, of the process as a whole (including both Field-work and Computation) is to choose Sites of *different cross-sectional figure*, the more unlike the better, (as happened in the present application.)

[In the preceding comparisons of Discharges *at same Site* (Test i) the same Float-Course Spacing and same formulæ were used in every pair of Results compared, so that no Discrepancies were introduced by the mere mode of computation. In what follows (Test ii), the Float-Course Spacing and formulæ differ at different Sites, so that Discrepancy is liable to be introduced in the computation: and it is possible that part of this Discrepancy *might disappear* if the Results were recomputed by other formulæ. The formulæ actually used were, however, in each case those believed to be the best with the available data, (Ch. XVII, 5a.)

6b. NORMAL DISCREPANCY.—The application of this Test consists in the comparison of the Discharge-measurements themselves.

[The use of the Mean Velocity-measurements in place of the Discharge-measurements is obviously not here admissible, (as under Test i); the former quantities not being proportional to the latter at different Sites].

And, it is clear that a small Loss is to be expected in passing from an Upper to a Lower Site from three different causes, viz., LEAKAGE, ABSORPTION and EVAPORATION. In what follows, GAIN and LOSS in passing from an Upper to a Lower Site will be indicated by a + and a — sign respectively. It would seem then that the normal Discrepancy should be steadily *negative* (indicating Loss), and that the occurrence of a positive sign (indicating Gain) can only be due to either—

1°. Influx of water into the Canal between the Sites, as from springs or inlets.

2°. Error in one or both of the Discharge-measurements, viz.,

(*a*), Under-estimation of the Discharge at the Upper Site,

(*b*), Over-estimation of the Discharge at the Lower Site,

provided of course that the Discharge-measurements be either of the same body of water, or else under similar External Conditions, in the Reach. Hence also a larger Discrepancy is admissible with a negative than with a positive sign.

As to the three efficient causes of Loss :—

1°. *Leakage.* This is of course a local matter.

2°. *Percolation.* This is known to take place largely all along the Canal, because the spring-level of the adjacent country has risen since its construction : this level was originally raised by, and is presumably still kept up by, infiltration from the Canal.

3°. *Evaporation.* The loss from this cause is too trifling to sensibly affect the Results (Ch. XXV, 8d).

7. **Test iia,** NON-SIMULTANEOUS DISCHARGES.—It will be seen from the Sketch Plan (Pl. I, 1) that the water from the Soláni Embankment passes next through the Soláni Twin Aqueducts, so that—

Discharge through Soláni Embankment Main Site = Sum of Discharges through the Soláni Twin Aqueduct Sites, ...(2).

The application of the Test consists in the comparison of the values obtained for these two quantities : with simultaneous Field-work at the Upper and Lower Sites, the Results should be equal; with non-simultaneous Field-work, the Results should be equal *only if obtained under the same External Conditions,* (an important limitation.)

Preliminary Work. Some preliminary trials were made in 1877 with the Staff (a single Field-party) available at the time, the Field-work being done as far as possible *in similar states of water* at the three Sites in succession. In two cases (Nos. 2, 3 of Tab. 28) the Field-work was done at all three Sites within the working hours of the same day, but this was found to be too fatiguing to carry on regularly. In the other cases, (when the Field-work at all three Sites was not completed within the same day,) several days sometimes passed between the work at the different Sites, partly from stress of weather, partly from the variable state of the Canal.

The Results are shown in Abstr. Tab. 28, which contains a simple Abstract of the data of this work from Det. Tab. XXXV—XLIV with slight additions. In consequence of the Field-work having been done on some occasions on different days in different states of water, the actual Discharge-measurements are not quite fairly comparable. To render the comparison fairer, a "correction" has been computed for the Results at the two lower Sites (Soláni Aqueduct) to reduce them to the Gauge-Reading of the Standard (Soláni Aqueduct) Gauge *recorded during the Field-work at the Upper Site.*

The correction (shown in Col. 7) is computed as follows :—

"Discharge-variation = Mean Velocity × Change of Area due to change of Gauge,

$$i.e., \frac{dD}{dh} \cdot \delta h = V \times \frac{dA}{dh} \cdot \delta h\text{''}, \ldots \ldots\ldots\ldots\ldots\ldots(3).$$

The sum of the Cubic Discharges through the Twin Aqueducts (with the above correction applied) is shown in Col. 7 underneath the Discharge at the Upper Site for comparison therewith.

7a. **Test iia,** DISCUSSION.—It will be seen that the Discrepancies are on the whole contrary to what was above described as "normal," thus—

1°. Three out of six show Gain (+ sign) in passing from the Upper to the Lower Site.

2°. The relative Gains are larger (as seen by the percentages) than the Losses.

Some of this abnormal discrepancy is due to the change of External Conditions in the Reach. Thus two of the Gains (Nos. 1 and 6) would be at any rate in part reduced if a correction could be applied to equalize the Surface-Falls ($F_1 + F_2$) in the Upper Sub-Reach. After making allowance for this, however, there will probably yet be a balance of abnormal Gain. And observing that there are no Inlets and no Springs between the Sites (being both in the Solání Embankment), this points to some *constant* source of Error in the Discharge-measurement tending to produce Gain on the whole.

Now the Discharge-measurements in the Lower Sites (Twin Solání Aqueducts) are known to be somewhat over-estimated in the Side-Spaces (Ch. XIX, 8b) in consequence of the neglect (in computation) of the slight contraction near the bed, and of the contraction under the corbelling. The reduction on this score is, however, too small in all the six cases under discussion to affect the Results much, and has not been thought worth computing, especially as the surface-contraction is least (only ·5 of a foot) in the case of largest Discrepancy.

A further probable source of over-estimation of the Results at these Lower Sites arises from the mode of estimating the depth, which was simply *assumed* to be the same as the reading on the Gauge, (the Gauge-zero being on the level of the Aqueduct-Floor,) thus making no allowance for the presence of Silt or occasional Obstructions (Ch. III, 12) on the Floor.

[The presence of $\frac{1}{10}$ foot of Silt on the Floor would cause over-estimation of from $2 \times 85' \times {\cdot}10 \times 3{\cdot}00$ to $2 \times 85' \times {\cdot}10 \times 3{\cdot}50$, *i. e.*, 51 to 60 cub. ft. per sec.].

8. Test iib, SIMULTANEOUS DISCHARGES IN SAME REACH.—The above application of this Test having given some confidence in the Results, it seemed desirable to give it a further trial in a more favorable way, viz., by having the Field-work carried out *at same time* at the Upper and Lower Sites, so as to eliminate the uncertainty of possible difference of the External Conditions when done at different times.

8a. *History of the work.*—A second Field-party was of course required for this purpose. The importance of this work appeared so great, that Government was pleased to sanction the necessary expenditure, and to detail two extra Overseers for it early in 1878.

The new men (Sergts. J. Tuer and G. Reynolds) joined in Feby. '78. Unfortunately, soon after their instruction was complete, the junior Observer (Corpl. G. Grey, R.E.) of the original Field-party—who was in fact only temporarily lent from the Bengal Sappers and Miners—was suddenly recalled to regimental duty: and notwithstanding the most urgent representations of the inconvenience necessarily entailed by his sudden removal, his services could only be obtained for a few days at a time at odd times during the three months of March, April, and May. Much of his time even at those odd times had necessarily to be given to bringing into proper form the computations connected with the Field-work he had been previously concerned in, (which might

otherwise have been entirely lost,) so that—including loss of possible Field-work days through stress of weather and sickness—there were eventually only 17 occasions in the whole three months (March to May 1878) in which it was possible for the two complete Field-parties to work in concert, thus giving in all 17 cases of Simultaneous Discharge-Measurement.

At the end of May 1878 Corpl. Grey and Sergt. Tuer were both finally removed, thus reducing the Establishment to a single Field-party again. But in November '78 it was increased again to two Field-parties, and in December '78 to three Field-parties for certain Experiments at a distance from Roorkee, described in Art. 9, *et seq.* The opportunity was taken whilst these parties were still in Roorkee of adding to the number of Simultaneous Discharge-Measurements in the Roorkee Reach. Eight more were done in all (Comparison Nos. 7 to 10, and 17 to 20 of Tab. 29, 30).

8b. Test iib, Application.—These Simultaneous Discharge-Measurements were done at the three principal Sites in the Roorkee Reach, viz., the Fifteenth Mile (Old and New), Soláni Embankment Main and Soláni Twin Aqueduct Sites. These Sites are fully described in Ch. III, 8—12c. Their relative positions (which is what most concerns this work) are shown in Pl. I, 1, and their Cross-Sections are given in Pl. II, 1, 2, 4.

It will be seen from the Sketch Plan (Pl. I), that the water passing through the Fifteenth Mile Sites, passes on through the Soláni Embankment Main Site, and lastly through the Soláni Twin Aqueducts.

[It should be noted that the Fifteenth Mile Site was completely remodelled (Ch. III, 8) between the earlier and later Experiments in 1878: (but though this affects the intercomparison of the Results at the Fifteenth Mile Site *among each other*, it in no way affects their use for the present purpose)].

The Field-work was done at two or more Sites at once by Field-parties working in concert at each; the velocity-measurements being commenced at nearly the same time at each Site; *the greater part of it was therefore executed within nearly the same working hours at each Site.* The work was distributed between the different Sites as follows:—

Sites.	1878 March to May. Reference No. [Tab. 29, 30].	Total.	Decr. 1878, and April 1879. Reference No. [Tab. 29, 30].	Total.	Grand Total.
Fifteenth Mile, Old Site, Soláni Embankment Main Site,	No. 11 to No. 16	6	No. 7 to No. 10	4	10
Soláni Embankment Main Site, Soláni Twin Aqueduct Sites,	No. 21 to No. 31	11	No. 17 to No. 20	4	15
Total, ..	...	17	...	8	25.

8c. Test iib, TABULATION OF RESULTS.—The Results are given in Tab. 29, 30, which contain a simple Abstract from Det. Tab. XXXIV—XLIX, with slight additions. The data which define the state of the External Conditions may be divided into two groups, General and Local.

i. *General*, defining the state of the Conditions in the whole Reach, viz.—

1°. Standard (Soláni Aqueduct) Gauge-Reading.

2°. Surface-Falls in Upper and Lower Sub-Reaches, ($F_1 + F_2$, and F_3).

ii. *Local*, defining local Conditions at each Site, viz.—

3°. Gauge-Reading, and Variation of same.

4°. Local Surface-Slope.

5°. State of the Wind at beginning and end of the Field-work.

In consequence of the Field-work being *simultaneous* at each of the Sites, none of these are likely to affect the Comparisons in hand in any way except the last, viz., the Wind; the way in which this is liable to affect the Comparisons lies of course in the increased difficulty of the velocity-work, and in the uncertainty of determination of water-level in a high wind.

In a few (three) of the preceding cases, viz., Nos. 8, 18; 9, 19; 10, 20 it is possible to exhibit this Test in a still more crucial manner, the Field-work having been done by three different Field-parties *working at the same time at the three Sites*. The Results are shown at foot of Tab. 30.

8d. Test iib, *Discrepancy in General Data.*—It might be expected that the General Data which define the state of the External Conditions for the whole Reach, viz.,

1°. The Standard (Soláni Aqueduct) Gauge-Reading,

2°. The Surface-Falls in the Upper and Lower Sub-Reach ($F_1 + F_2$, & F_3),

should be the same at each Site for any one Comparison, in consequence of the simultaneity of the Field-work, whereas there are slight differences in the values recorded for each Site.

It seems necessary to explain this. The entries of the Standard Gauge-Reading are readings obtained by each Field-party *independently** *for itself* at the time of passing the Gauge on going out to or returning from Field-work, and therefore at different times. The reading of this Gauge at different hours, and therefore often in different states of wind, accounts for the slight differences in the entries for each Site. Further, the Surface-Falls in Upper Sub-Reach (Head-Gauge to Standard Gauge), and in Lower Sub-Reach (Standard Gauge to Tail-Gauge) depend also on this Standard Gauge-Reading: this accounts for the slight differences in the entries for each Site; any considerable differences must be due to real changes going on in the state of the Canal.

8e. Test iib, *Discrepancy in Wind.*—It might be supposed that since the Field-work is said to be *simultaneous*, the Wind entries ought to be the same for all the Sites. But, firstly the Wind-observations were *not synchronous*: those at the beginning of the Field-work were done only roughly speaking about the same time, whilst those at the end were done for each Site at the end of the Field-work of that Site, and therefore at very different times (differing sometimes by about an hour): Next, the exposure of the Sites was very different: the Fifteenth Mile Site is in a

* it being thought best that each party's work should be quite independent.

deep cutting, the high banks of which are well planted with trees, so that it is well sheltered from wind; whereas all the Solání Sites are on the top of the lofty Solání Embankment across a wide open valley (2¼ miles wide), without shelter of any kind from the wind.

This explanation will sufficiently account for the great differences in the Wind record at the different Sites. It may be noticed that at the Sites with similar exposure (the Solání Sites), there is commonly a fair similarity between the *first* Wind-Observations which were made about the same time, also that the Wind-entries for the sheltered (Fifteenth Mile) Site are steadily less than for the exposed Solání Embankment Site.

8f. *Unequal Discharges in Twin Aqueducts.*—It might be expected that the Field-work having been on each occasion continuous (beginning at left bank of Left Aqueduct, and ending with right bank of Right Aqueduct),—and lasting only about 4 hours, the Discharges through both Aqueducts should have been equal. But on referring to Tab. 30, it will be seen that the Right Aqueduct Discharge-Measurement is usually markedly the larger.

[There is only one contrary instance (No. 23, and that is an unimportant one) in the Total of 15 cases (Tab. 30)].

A similar excess occurs in 5 out of the 6 Cases in Tab. 28 of Non-simultaneous Field-work.

This excess is no doubt due to the partial obstruction of the Left Waterway (noticed in Ch. III, 12b & XVII, 12, i) in the imperfect removal of the dams used for repair.

8g. Test iib, *Discussion.*—**As a preliminary to tracing out the causes of the Discrepancies (some +, *i.e.*, gain: some —, *i.e.*, loss) between the Discharges at the different Sites, it will be well to recapitulate the condition of the bed of the several Sites, and the state of water in which the Soundings were made at each Site.**

It should be remembered that—

1°. A level bed is favorable to the use of velocity-rods, and also to sounding.

2°. A rough bed is unfavorable to the use of velocity-rods, and tends (Ch. XV, 11a) to over-estimation of the Mean Velocity past each vertical.

3°. Soundings in deep swift water are unfavorable, and tend to over-estimation of the depths, (from the difficulty of holding the Sounding-Rod upright).

When both conditions 2°, 3° *are combined*, the Discharge may fairly be expected to be over-estimated. The Table next below is an Abstract of these conditions at each Site, and of the effect on the computed Discharge.

[There is also a *small* Leak a little below Mahewar Bridge, *i.e.*, below the Fifteenth Mile Site and above the Lower Sites, probably too small to affect the Results much: similarly the amount of percolation between these Sites is too small to be worth notice].

SITE.	March to May 1878.	December 1878 to April 1879.
Fifteenth Mile,	Bed very rough. Soundings in deep swift water. *Result.* Discharge over-estimated.	Bed pretty level. Soundings in deep swift water. [No inference as to effect on Discharge].
Soláni Embankment, [Main Site].	Bed rough. Soundings in shallow slack water. [No inference as to effect on Discharge].	Bed rough. Soundings in deep swift water. *Result.* Discharge over-estimated.
Soláni Twin Aqueducts,	Bed level, but with occasional accidental heaps of mud, stones, &c. Depths inferred from the Gauge-reading ; probably over-estimated, as no deduction is made for silt deposit, (Ch. III, 12, 12a). Discharge certainly over-estimated in the Side-spaces, (Ch. XIX, 8b.) *General Result.* Discharge somewhat over-estimated.	

From this it is clear that in comparing Discharges at any two of these Sites, the Discrepancies may be expected to be Gain (+) or Loss (−) as shown in the following Table. The Table shows also an Abstract of the Actual (percentum) Discrepancies taken from Tab. 29, 30.

SITES.	SEASON.	Discrepancy expected. [Gain +, Loss −]	Experimental Results.			
			Comparison No. [Tab. 29, 30]	Total.	Discrepancy.	
					Sign.	Range. [per cent.]
Fifteenth Mile, & Soláni Embkt.,	March to May '78,	Loss	Nos. 11 to 16	6	All −	− 8·3 to − 2·7
	Decr. '78 to April '79,	Gain	Nos. 7 to 10	4	3 +, 1 −	+ 5·8 to − 1·6
„ Embkt., & „ Aqueduct,	March to May '78,	Gain	Nos. 21 to 31	11	All +	+ 5·2 to + ·3
	Decr. '78 to April '79,	? Loss	Nos. 17 to 20	4	3 −, 1 +	− 4·8 to + ·3

The agreement between the signs of the Discrepancies expected *à priori* and actually obtained by Experiment will be seen to be very close, there being only 2 exceptions in the 25 cases.

The larger Discrepancies also occur mostly (viz., 7 out of 9 over 4½ per cent.) in the work connected with the Fifteenth Mile Site, (Tab. 29.) This is probably due to the roughness of the bed at that Site.

Lastly, it will be seen that there were Causes at work (*see* Notes in right hand Col. of Tab. 29, 30) tending to produce uncertainty in the Results, viz.—

1°. Unsteadiness of the water in 5 cases shown either by the Gauges at the Sites (Cases 20, 21), or by the large discrepancy in the Surface-Falls of the Upper or Lower Sub-Reach as registered for each Site, (Cases 8, 9, 18), and—

2°. High wind in 4 Cases (10, 15, 20, 21).

The general smallness of the proportionate Discrepancies (few of which are of practical importance), taken together with the fact that they are to a great extent satisfactorily accounted for by the conditions of the time, points to the conclusion that the agreement would have been in all proba-

bility a good deal closer under more favorable Conditions at each Site, and is decidedly in favor of the general accuracy of the process (of Discharge-Measurement) used, when done under really favorable circumstances.

9. **Tests iic,d, Discharges in different Reaches.**—These Tests were applied on a large scale at beginning of 1879 at the three Discharge Sites in the Belra, Jaolí, and Kamhera Reaches, which are described in detail in Ch. III, 13—17, and Pl. IV—VI, and whose relative position—which most affects the present question—is shown in Pl. III, and described in Ch. III, 13, 13a.

It will be seen that the water passing through the Belra Reach is divided (at the Jaolí Regulator), so that part passes down the Jaolí Reach, and part down the Kamhera Reach, except only for a small quantity, which is diverted near the Tail of the Belra Reach into four small Distributary channels (the Right Jaolí, Mansúrpur, Mírápur, and Pimora).

It follows therefore that, except for evaporation and percolation—

"Discharge at Belra Site = Sum of the Discharges at Jaolí and Kamhera Sites and in the four Distributaries",..(4).

From these two Tests (iic,*d*) having been applied at the same Sites, it will be convenient to discuss them together in following Articles (Art. 9a—h).

9a. *Sites unfavorable.*—It cannot be said that the Sites were favorable, either individually or collectively, for these Tests. The faults of each of the three large Sites have been explained (Ch. III, 14—16) as being chiefly—

1°. *Belra Site.* Too near Belra Bridge, Bed-level scoured below level of Jaolí Falls Crest, Bed rough.

2°. *Jaolí Site.* Too near Jaolí Regulator Falls, Bed very rough.

3°. *Kamhera Site.* Bed and Banks rough.

The faults of these Sites collectively were chiefly—

1°. Their distance apart (about 6½ miles from Belra to Jaolí Site, and 2½ more to Kamhera Site) increasing the uncertain amount of Loss from leakage, absorption, and evaporation.

2°. The existence of the four small Distributary channels leading out from the Belra Reach near its Tail, (Pl. III, & Art. 9g).

9b. Range of Work.—Notwithstanding these disadvantages, the Comparisons in question are believed to be of great value as a Test of the fair accuracy of the process used from the grand scale of operations, and from the considerable Range of water-level and of Discharge at each Site, as follows:—

At Belra—Gauge Reading, 7·'54 to 5·'74; Range, 1·'80.
Discharge, 5,751 to 4,166 *c. ft. per sec.*; Range, 1,581.

At Jaoli—Gauge-Reading, 7·'32 to 5·'21; Range, 2·'11.
Discharge, 4,813 to 3,202 *c. ft. per sec.*; Range, 1,611.

At Kamhera—Gauge-Reading, 6·'56 to 5·'27; Range 1·'29.
Discharge, 982 to 711 *c. ft. per sec.*; Range 271.

Four Distributaries—Total Discharge 298 to 0 *c. ft. per sec.*; Range 298.

[The above is an Abstract simply of the highest and lowest quantity of each kind: the Results are *not simultaneous*].

9c. Tests iic, d, Application.—The proper application of the Test-equation (4) above appears to require either—

Test ii*c*. The Field-work at all the Sites should be done *nearly at the same time*, (*i.e.*, begun about same time, and finished about same time).

Test ii*d*. The Field-work should be performed as far as possible *upon the same mass of water*: (being begun later at the lower Sites).

Both Methods were tried as follows:—

Test ii*c*. This Method was adopted throughout the month of January 1879, the Field-work being begun about the same hour at all the Sites.

Test ii*d*. This Method was adopted throughout the months of February and March 1879. The probable Mean Velocities at the different Sites being (roughly) known from the January work, the Field-work of each day was begun first at the upper (Belra) Site; the Field-work at the two lower (Jaolí and Kamhera) Sites was begun after an interval, supposed to be sufficient to admit of some of the mass of water (which was at the upper Site at the beginning of the work) passing down to and reaching the lower Sites. The allowance is of course very rough, being only guessed at from the probable Mean Velocities.

[This second Method can only be adopted in cool weather: its adoption in hot weather would involve the Field-work at the lower Sites being done in the heat of the day, (if that at the upper Site be done in the cool hours of the early morning), whenever the Sites are—as in the present instance—several miles apart].

9d. Staff.—Three Field-parties, each complete in itself, were employed (working in concert) at each of the three principal Sites (Belra, Jaolí, and Kamhera); four additional Overseers having been detailed for this special work by Government, (over and above the two in permanent employ). The new men joined in November and December 1878, and were trained in Roorkee; their training was finished in December; and the whole party moved out into camps formed near the several Sites at the end of 1878.

The first few days of 1879 were employed in the preliminary levelling operations (Ch. VII, 4) necessary for Surface-slope measurement: this covered a good many days, as there was only one large Level available suitable for this purpose.

The systematic work—at all the Sites in concert—was in full operation from 9th January 1879, and was continued daily (on week days) with few exceptions from stress of weather, sickness, &c., till end of March 1879, when camp was broken up, and the parties returned to Roorkee to reduce their work: this occupied the month of April; at the end of April, the extra Staff was all dispersed.

There was at times so much sickness among the Staff as to threaten the loss of much of the systematic work, (which it will be seen required the *simultaneous* work of all six Overseers). An additional Overseer was accordingly applied for as "wait-

ing man", and joined in February 1879, but he proved unfortunately the sickliest of the whole.

[The sickness was not caused by the Experiments' work; the men themselves were in bad health when they joined].

The number of Discharge-measurements made in this time at these Sites was—

"Belra Site, 53; Jaolí Site, 55; Kamhera Site, 56."

Out of these there were 44 that were done in concert, as above explained.

The Staff was thus arranged for the systematic work; the distinguishing initial of each is shown alongside his name.

Superintendent, Capt. Cunningham (C).

Observers, at Belra—Sergt. W. Porters, (P), and Mr. J. Andrews, (A).

Observers, at Jaolí—Sergt. G. Reynolds, (R), and Mr. C. P. Smith, (S).

Observers, at Kamhera—Mr. J. Clowsley, (Cl.), and Mr. J. Callaghan, (C).

Observer, (Waiting Man)—Sergt. J. Bell, (B).

Clerk and Computer—Mr. G. Henry, (also employed in silt-collecting.)

Besides the *regular* Discharge-work at the three principal Sites, which fully occupied the 6 Observers detailed to them, some extra hydraulic work was undertaken by the 3 remaining hands—in addition to their ordinary duties, viz.—

The Superintendent and Sergt. Bell formed a fourth Observing party, partly for Discharge-measurement in the 4 Distributaries, and partly in trials of Current-Meters. A daily silt-measurement was undertaken by Mr. Henry. This work will be described in Ch. XXIV, 9b.

The whole of the Field-work (down to the minutest particulars) was done *under the immediate personal supervision* of the Superintendent, who accompanied* the parties out into the Field, and was present with one camp or the other nearly all the time.

9e. Tests iic,d, Tabulation of Results.—The Results are given in Abstr. Tab. **31**, as follows:—

Test iic, see Comparison Nos. 32—41 of Table.

Test iid, see Comparison Nos. 42—75 of Table.

They are simply an Abstract from Det. Tab. L—LV. It has not been thought necessary to give quite such full detail as in the former Comparison-cases.

[The Reference number to the Series and Set in the Detailed Tables has not been given, as they can be easily identified by the Date and Gauge-Reading: the Tables are—

Belra Site, Tab. L, LI; Jaolí Site, Tab. LII, LIII; Kamhera Site, Tab. LIV, LV.

Again, the Fall of Water-Surface and Surface-Slope have not been given in the present case, as from the circumstance of the Field-work having been done *in concert* at all the Sites, it in no way affects the question of Comparison of the Discharge-Results].

The record of the state of the Wind at beginning and end of the Field-work is given solely to show the different circumstances at each Site, the Field-work being more difficult, and therefore less trustworthy in a high wind.

9f. *Discrepancy in Wind.*—It will be seen that the Wind entries are usually *very different* at the different Sites: the causes of this are (as in Art. 8e)—

* having been temporarily relieved of his work in the Thomason C. E. College for this purpose.

1°. Non-synchronism of the wind-observations.
2°. The distance apart of the Sites.
3°. The different exposure of the Sites.
4°. The different directions of the Reaches.

The wind-observations not being synchronous (1°), a general change of wind might occur in the interval, which might take effect at a different time at each Site, in consequence of (2°) their distance apart. Next, (3°) the exposure of the Sites was very different, as follows :—

Belra Site, between high banks, thickly planted with trees on both sides, and therefore well sheltered from cross (*i.e.*, E. or W.) wind.

Jaoli Site, between high banks, planted with trees on left bank only, and therefore not much sheltered from the prevailing wind, (usually W., *i.e.*, from R. bank.)

Kamhera Site, Low banks, very little shelter.

The wind-entries show accordingly almost without exception far less wind at the Belra Site than at the other Sites ; and that chiefly up- or down-stream.

Lastly, (4°), the direction-entries show direction (not cardinal direction, but) in a special way, viz., referred to the axis of the stream at each Site as the working N. S. line (Ch. V, 21) : so that a wind of the same real cardinal direction would be *registered with a different "direction" at each Site,* in consequence of the different directions (*see* Pl. III) of the three Reaches.

9g. *Distributary-Discharges uncertain.*—The presence of the Distributaries was disadvantageous, as introducing an element of uncertainty into the Comparison Results. The proper application of this Test would obviously have required either the complete closing of the Distributaries, or else simultaneous measurements of the Discharge through all four made in concert with those of the three principal Sites. The irrigation necessities of the country would not admit of the former ; and to have effected the latter would have required the addition of four more complete Field-parties, an expense which could not be incurred.

By a special arrangement with the Canal Staff, the whole of the four Distributaries were, however, closed for five days (24th to 28th Febry.), and were also run very low on other six days 15th, 17th, 24th to 27th March. The Comparison Results on these days have therefore a special value.

In order to make some allowance for the Discharge passing through the Distributaries, the Gauge-Reading of each Distributary was taken daily (by the Canal Staff). The Discharges corresponding were taken out from the official Canal Tables ; the sum of these four Discharges is entered daily in each Comparison for the purpose of making up the Total Discharge passing through the Lower Sites for comparison with that passing through the Upper (Belra) Site.

This of course introduces an element of uncertainty into the Comparisons, (the sum of the Tabular Discharges of the Distributaries amounts at a maximum to about 5 per cent. of the Total Discharge of the Canal,) but it was the best allowance that could be made under the circumstances.

In order, however, to afford some check upon the figures so obtained, a few (9) Check Discharge-measurements were taken in the four Distributaries by the fourth Field-party (mentioned in Art. 9d). The Results are shown in Tab. 23 for comparison with the figures given in the official Canal Tables ; the latter will be seen to be generally markedly the lesser of the two : as to the cause of this, *see* Ch. XIX, 20d, e.

9h. Tests iic,d. Discussion.—The Table below is an Abstract (from Tab. 31) of the resulting Discrepancy between the Discharge at Upper Site and the Sum of the Discharges at the Lower Sites.

It will be seen that the Results are decidedly closer when the Field-work is done *upon the same body of water* (Test ii*d*), than when the Field-work is *simultaneous* (Test ii*c*); the Discrepancies in the former case being nearly all on the side of Loss, which has been explained (Art. 6b) to be the normal Result.

Description of line.	Test ii*c*. [Simultaneous Field-work]. 10 cases.	Test ii*d*. [Field-work in same water]. 34 cases.	Total. 44 cases.
Range, ..	+ 3·5 to − 2·7 °/ₒ,	+ 2·3 to − 5·5 °/ₒ,	+ 3·5 to − 5·5 °/ₒ
Number of cases,	5 gain, 5 loss, 3 over 2 °/ₒ gain, 3 over 2 °/ₒ loss, [Gain (+), Loss (−)].	5 gain, 29 loss, 2 over 2 °/ₒ gain, 5 over 3 °/ₒ loss, [Gain (+), Loss (−)].	10 gain, 34 loss. 5 over 2 °/ₒ gain. 5 over 3 °/ₒ loss. [Gain (+), Loss (−)].

As part explanation of the Discrepancies, it may be seen (from the right hand Col. of Tab. 31) that the Unsteadiness of the water in 4 Cases (Nos. 55, 66, 67, 75), and High Wind in 18 cases must have tended to produce uncertainty in the Results.

The general Result is very satisfactory, the number of cases exceeding 2 per cent. of gain or 3 per cent. of loss being very few, only 5 of each out of the whole 44. And there is no doubt that the Discrepancies which show Loss would be generally somewhat less had the Discharges in the Distributaries been measured by the same process as used at the large Sites: for the few Check Discharge-measurements taken in the Distributaries givé Results generally larger than those of the Canal Tables, (*see* Tab. 23.)

There does not appear to be any obvious cause for the occurrence of cases of Gain (+) at all. These are, probably in part, due to error in the Results. It will be seen that in the 6 cases (Nos. 55 to 60) in which the Distributaries were closed, the Discrepancies are all on the side of Loss.

10. Abstract of Tests.—The Test applied to verification of the Discharge-measurements has been very varied and searching: the following is an Abstract:—

Test i*a*. *Results in same Series.* 78 Series containing 559 Results: the number of *pairs* of Results thus virtually compared is of course very large. Only 14 cases of Discrepancy exceeding 10 per cent., and these all accounted for by the varied External Conditions.

Test i*b*. *Results of same day at same Site.* Total 106 groups of Results, *i.e.*, 190 Comparisons. Only 11 cases of Discrepancy over 5 per cent., and these all accounted by the varied External Conditions; also only 2 more over 4 per cent., and 16 more over 3 per cent., and these partly accounted for by the change of External Conditions.

Test i*c*. *Simultaneous work on different Runs.* One Result on each of 4 different Runs. Results of 50′ and 100′ Runs closely accordant.

Test ii*a*. *Non-simultaneous work in same Reach.* Total 6 comparisons. Only one case of Discrepancy over 4½ per cent., and this partly accounted for.

TEST ii*b*. *Simultaneous work in same Reach.* Total 25 comparisons. Only 6 cases of Discrepancy over 5 per cent. Most of the cases of Gain partly accounted for.

TEST ii*c*. *Simultaneous work in different Reaches.* Total 10 comparisons. No Discrepancies amounting to 3 per cent. Only 5 cases of Gain. Discrepancies partly accounted for.

TEST ii*d*. *Discharges of same body of water in different Reaches.* Total 34 comparisons. Only 5 cases of Discrepancy over 3 per cent. Only 5 cases of Gain. Discrepancies partly accounted for.

11. Causes of Discrepancy.—The various causes of Discrepancy in the above Comparisons would seem to be—

1°. Unfavorable condition of the Sites.

2°. Unfavorable state of External Conditions, (*e.g.*, High Wind, Canal being "out of train", &c.)

3°. Dissimilarity of External Conditions (in the Non-simultaneous work).

4°. Unsteady Motion of the water.

5°. Observation-Error, including personal equation.

6°. Use of different formulæ.

After making every allowance for the first three and the last (which have been already considered) there will remain a certain amount of residual Discordance, which must be ascribed to Nos. 5°, 6°. From the care taken in the actual observations, it seems probable that the ordinary Observation-Error (in the final Result) must be very small, so that most of the residual Discordance is probably due to the Unsteady Motion of the water. This confirms the view before advanced (Ch. VI, 16) that—

"Single Discharge-measurements (depending on say only 50 separate velocity-measurements) are Fair (but not close) Approximations",......................(5).

12. General Conclusions.—Taking the evidence as a whole, the following Conclusions seem fair :—

"Under favorable circumstances the process of Discharge-measurement described in this Work yields consistent Results",......................................(6*a*).

"The amount of discordance between successive Results may be expected to be seldom over 3 per cent.",..(6*b*).

Viewing the very great variability of velocity at any one point arising from the Unsteady Motion of the water, the above Conclusions are very satisfactory, and are *of great practical importance.* To secure, however, anything like the above close approximation, it is obviously necessary that the "favorable circumstances" mentioned be secured, viz.—

"The condition of the Sites must be favorable",..................................(7*a*).

"The velocity-measurements should not be done either in high wind, or when the Canal is not 'in train'",..(7*b*).

"When successive Results are to be compared, close similarity of External Conditions is essential",..(7*c*).

CHAPTER XXII.

PRACTICAL DISCHARGE-MEASUREMENT.

***Preface.*—This Chapter contains the System of Cubic Discharge-Measurement proposed for practical adoption, and may be looked on as the chief practical outcome of the whole Work to which all that precedes has led up.**

1. Practical Discharge-Measurement.—The most important *practical* Result of the whole of this Work is conceived to be the elaboration of a system of Discharge-Measurement proved to have the merit of giving consistent results on a large scale, which can be carried out on a large Canal in a few hours, and requires the use of such a simple Instrument as the Velocity-Rod.

It remains to offer a somewhat simpler modification of the details (than the laborious system thought necessary for these Experiments) for general practical application. It will be obvious from the description of the system of Sounding and Discharge-Measurement described in Ch. V, 13—17; XVII, 5—7; XIX, 1—15a, that the amount of labor depends chiefly on two things—

1°, the number of Cross-sections taken,

2°, the number of Float-Courses adopted,

and can only be reduced by reducing the number of one or both of these. Opinions will differ as to the number of each necessary and sufficient for the approximation required. All that can be said is that the greater the number, the greater the labor and, the greater also the probable accuracy of the Result (supposing of course that the Cross-Sections and Float-Courses are well selected).

2. Cross-Sections.—At least three Cross-Sections should be taken, one at the centre of the Site, and one at each Rope. Any others that time admits of taking should be in pairs at equal distances from the centre of the Site. The Soundings to be taken along the Float-Courses, (*see* next Article).

3. Float-Course Spacing.—The principles for arranging this are

explained in Ch. XVII, 5—6. It is recommended that there should be always—

One Float-Course at centre.

One close (as close as possible) to each bank, when the banks are vertical.

One over foot of each Side-slope, when the banks are sloping.

The sub-division of the Central Space must depend on the time available. The advantage of "Weddle's Rule" is so great (both in accuracy and convenience), that the Float-Courses should be arranged to suit it whenever time permits. Its use requires division of the Central Space into 6 *equal parts*, making in all (along with the two outer Float-Courses above-mentioned) a total of 7 Float-Courses,—a number which should not unduly tax the practical Engineer.

4. Field-work.—This consists of two parts—

i. *Sounding.* The AVERAGE DEPTHS (H) along each Float-Course to be found (by sounding).

ii. *Velocity-work.* The ROD-VELOCITIES (u) in each Float-Course to be taken, each being at least thrice measured.

5. Computation.—In following scheme the same accents, and subscripts are given to the quantities H, u, D of same Float-Course, (where D = Superficial Discharge past the corresponding vertical); the subscript cipher ($_0$) indicates the centre, the accents indicate Right or Left of Centre.

Side-Space.				Central Space.				Side-Space.
[Width β''].				[β = Width of each segment of Central Space].				[Width β'].
?0	u_3''	u_2''	u_1''	u_0	u_1'	u_2'	u_3'	?0
H_4''	H_3''	H_2''	H_1''	H_0	H_1'	H_2'	H_3'	H_4'
?0	D_3''	D_2''	D_1''	D_0	D_1'	D_2'	D_3'	?0

STEP I. Compute the product (D) of every H by the corresponding u, (*i. e.*, $D = Hu$), and observe that the Edge-velocities being assumed zero, the values of D at the edge are also zero; also that the depths (H_4', H_4'') at the edges would be zero with sloping banks.

STEP II. The Cubic Discharge in the Side-spaces (say $\mathbf{D}'$, $\mathbf{D}''$) and Centre-Space (say $\mathbf{D}_0$) must be computed separately, thus—

Centre-Space, $\mathbf{D}_0 = \frac{3}{10}\beta \times \{(D_3'' + D_1'' + D_0 + D_1' + D_3') + 5(D_2'' + D_0 + D_2')\}$

Side-Spaces, (vertical banks), $\mathbf{D}'' = \frac{2}{3}\beta'' D_3''$, $\mathbf{D}' = \frac{2}{3}\beta' D'$.

" " (sloping banks), $\mathbf{D}'' = \frac{1}{3}\beta'' D_3''$, $\mathbf{D}' = \frac{1}{3}\beta' D'$.

Lastly, Total Cubic Discharge, $\mathbf{D} = \mathbf{D}'' + \mathbf{D}_0 + \mathbf{D}'$.

STEP III. The Area (if required) should be computed by similar formulæ (so as to give equal approximation), thus; (A'', A_0, A' being the Areas of the Left Side-Centre-, and Right Side-Spaces),

Centre-Space, $A_0 = \frac{3}{10}\beta \times \{(H_3'' + H_1'' + H_0 + H_1' + H_3') + 5(H_2'' + H_0 + H_2')\}$

Side-Spaces, (vertical banks), $A'' = \frac{1}{2}\beta'' . (H_4'' + H_3'')$, $A' = \frac{1}{2}\beta' . (H_3' + H_4')$

" " (sloping banks), $A'' = \frac{1}{2}\beta'' . H_3''$, $A' = \frac{1}{2}\beta' H_3'$

Lastly, Total Area, $A = A'' + A_0 + A'$.

6. Details.—For further information as to details, *see* following :—

1°. *Choice of Site*, Ch. I, 7; XV, 11—11b: *Preparation* (for use of Rods), Ch. XV, 12.
2°. *Ropes*, and *Pendants*, Ch. IV, 14—18.
3°. *Sounding*, Ch. V, 16, 17; *Average Depths*, Ch. V, 13—15.
4°. *Rods* and *Plank Trays*, Ch. XV, 4—7f.
5°. *Timekeeper*, Ch. IV, 26b.
6°. *Timing*, Ch. IV, 26, 26a.
7°. *Length of Run*, Ch. IV, 27, 30.
8°. *Good Floats*, Ch. IV, 31, 31a.
9°. *Field-Book*, Ch. IV, 32, 33, and Tab. 33.
10°. *Velocity-reduction*, Ch. IV, 34.

7. Special Notes.—Upon the above matters, the following should be noted :—

1°. *Sites.* The nearer a Site conforms to the principles laid down the better.
2°. *Ropes.* To be strained as near the water-surface as possible.
3°. *Sounding.* To be done from a boat floating freely down-stream, if possible with a Sounding-Rod, (not with a "sounding-line".)
4°. *Rods.* Tube-Rods are the best: a Set of 12 of each length in most frequent use, and of 6 of all other lengths will be required; the lengths to advance by 6" from a length of 1' upwards.

[If the Depth exceed about 15', the use of Rods would be impracticable. In this case the Double-Float is recommended, sunk as a general rule to ⅘ depth in each Float-Course; (*see* Ch. XIV, 18)].

5°. *Timekeeper.* A half-second's chronometer is best. Next to this (and nearly as good) a loud ticking clock (not ticking too quick for the ear to follow) with second's hand. Next a similar clock without second's hand, or else a metronome. Next a stop-watch.

Next probably (but much inferior) a simple seconds or half-second's pendulum. And last of all a common watch or clock provided with a second's hand.

[The pendulum may be improvised with a bullet and string: its proper length must be found by comparison with a good watch or clock *through at least a minute*].

6°. *Timing.* The essential feature is that (to eliminate personal equation) the similar work at either Rope should be done by the same person, *i. e.*, one person must "call" at both Ropes, and another must "keep time" throughout.
7°. *Length of Run.* The better the Timekeeper, the shorter the necessary Length of Run; and *cæteris paribus* the shorter the Run, the better. A 50-foot Length suffices with a good Timekeeper; a 200-foot Length would probably suit a common watch.
8°. *Good Floats.* The sole criteria are that a Float should run "free" and in "fair course". Irregularity of time in successive Floats is *no drawback*, but is to be expected.
9°. *Field-Book.* None but "Good Floats" to be recorded.

8. Rapid Approximation.—As to the course recommended when

time does not admit of velocity-measurements on many verticals, (which is considered to be *by far the best* Method,) *see* Ch. XX, 29, where Central Mean Velocity-measurement repeated about 50 times is proposed as an alternative.

[For a succinct explanation of the process of Discharge-measurement above advocated (Art. 1—7), *see* the Author's Paper on Discharge of Canals, in Profl. Papers on Ind. Engng., Vol. VI of '77, p. 286. It is not thought necessary to recapitulate the process in detail here].

END OF PART III.

PART IV.

CHAPTER XXIII.

CURRENT-METER WORK.

***Preface*.—This Chapter is mainly an Account of the difficulties (Art. 3—4) found in the use of Current-Meters, and also of proposed improvements in them (Art. 6—11).**

1. No systematic Experiment.—The work actually done with Current-Meters in these Experiments can only be described as preliminary and tentative. In fact the TARAGE (or experimental determination of the meaning of the graduations at different velocities) was not undertaken, so that no systematic velocity-work could be done. The Ganges Canal is not very favorable for the use of Current-Meters from the frequent presence of bits of weed, and other *flotsam* in the stream. In the face of the uncertainties attending the use of these Instruments (Art. 3—3, xiii), and the strong disapproval expressed by such experienced hydraulicians as Genl. Abbot (Art. 3, xii), the Author did not feel justified in spending more time on them, when velocity-work with Floats was urgently required.

Of all the trials made, very few have accordingly been made use of in the Discussions in this Work; a few will be found in Tab. LXXIV illustrating Unsteady Motion; and a few are alluded to in this Chapter. For the purposes there required the *tarage* was not essential.

This Chapter is only intended to discuss the objections to the use of these Instruments, and the improvements that might be effected both in the Instruments themselves and in the way of using them.

2. Current-Meters tried.—Three Current-Meters of different pattern were supplied for use on these Experiments, viz.—

1°. ***Moore's Current-Meter*, *see* Civ. Engr. Inst. Procgs., Vol. XLV, Paper No. 1481.**
2°. ***Révy's Current-Meter*, *see* Révy Report, p. 155.**
3°. ***Roorkee Current-Meter*, similar to Elliott's Current-Meter (*see* Madras Manual of Hydraulics, 3rd Ed., Art. 35).**

A full description of these Instruments will be found in the Works quoted: as they were not used for any continuous work, it seems unnecessary to give detail here.

3. Objections.—The following are the chief objections to Current-Meters of all sorts :—

- i. Disturbance of the natural motion of the water, (caused by the presence of the Instrument itself, of its mountings, and of the boat from which it is worked.)
- ii. Uncertainty of orientation.
- iii. Uncertainty of position, (in some modes of use.)
- iv. Uncertainty of gearing and ungearing, (in the ordinary use.)
- v. Non-measurement of "forward velocity".
- vi. Failure to work at or near the surface.
- vii. Failure at low velocities.
- viii. Delay involved in raising the Instrument to the surface to read.
- ix. Liability to be choked by weeds.
- x. Invisibility when at work.
- xi. Expense.
- xii. Delicacy, causing great liability to injury.
- xiii. Uncertainty of record.
- xiv. Faults of construction.

Certain of these faults can be more or less got rid of by the following:—

- 1°. By the use of a special Current Meter-Lift described in Art. 6, 7: this removes almost entirely all the objections Nos. ii, iii, iv, v, and to a great extent the disadvantages of Nos. i, x.
- 2°. By the separation of the "recording works" as described in Art. 10: this improvement removes objections Nos. iv and viii, and reduces Nos. i and x.

Here follows detail of the above—

[To facilitate reference, the Articles discussing the several Objections i—xiv bear the same (subordinate) numbering, viz., i—xiv].

3, i. DISTURBANCE OF WATER.—This is a fault inherent in all Fixed Instruments. Some Current-Meters are so bulky (Moore's is about 19½" × 6" × 5½") as to cause considerable obstruction in the stream.

The use of a boat or pontoon-raft from which to work the Instrument is a necessary evil in a wide channel (in the absence of a bridge): if this boat or raft has to be moored by anchoring, the mooring apparatus is an additional obstruction.

The mounting arrangements (for securing the vertical raising and lowering of the Instrument) form a further obstruction, and in some cases a very serious one. One illustration will suffice.

[*Mounting Gear, Révys.* The "mounting" recommended in the Révy Expts., (pp. 17 and 158) for general use in depths exceeding 10', consists of an iron bar about 11' × 3" × ½" to be suspended from the Observatory Raft in a horizontal position parallel to the current-axis by two ropes, by which it can be raised or lowered in such a way as to preserve its horizontality. The Current-Meter is attached a little below the up-stream end of this Bar. The ends of the Bar are to be connected by cords

to two boats moored, one above and one below the Observatory-Raft, at from 50 to 100 yards distance from it. This connexion with the moored boats may be so managed as to secure the approximate verticality of the two suspension-ropes during the raising and lowering, and therefore also ensures the motion of the Current-Meter in a tolerably vertical line. This apparatus was found quite successful in the Révy Experiments.

The advantages are, however, gained at the expense of introducing a serious amount of obstruction into the water, viz., the iron bar with certain appendages not detailed above, two suspension-ropes, two long connecting cords, and the moorings of three boats, two of which require double moorings.

[It may perhaps be impossible to use Current-Meters in deep water without some such lowering apparatus as above; in this case the obstruction introduced is a serious objection to the use of Current-Meters at all].

Altogether the obstruction caused by the Instrument itself, by its "mountings", by its boat or raft, and by its moorings, is liable to be very great, and must greatly interfere with the trustworthiness of its indications.

3, ii. Uncertainty of orientation.—All the Current-Meters known to the Author are supplied with a large "Tail", consisting either of a single vane in a vertical plane, or of a pair of vanes at right angles.

In the former (which is the usual) construction, the Instrument is intended to be freely pivoted on a vertical axis, so as to be free to turn *in a horizontal plane.* In the latter construction (which is adopted for Moore's Current-Meter), the Instrument is intended to be freely pivoted, so as to be free to turn *in all directions* (in any plane).

In either construction, the intention is to avoid the practical difficulties of *fixing* the Instrument in direction by utilising the action of the "Tail" to secure the constant presentation of the blades of the fan to the stream.

In consequence, however, of the Unsteady Motion of the water, the Instrument is kept constantly swaying about, following to some extent the variations (in direction) of the stream-lines passing it. The Instrument does not of course change direction to the same extent as the individual stream-lines, partly from the friction on its pivots, (which prevents its turning quite freely,) and partly from the partial neutralization of the pressures on opposite sides, the momentarily unbalanced portion of which is the sole cause of its shifting at all. This momentary oscillation is similar to the oscillation of a Wind-vane.

The Average of these momentary directions for any one point in a channel may be *assumed* as the Average direction of the stream at that point. But there is no certainty that this Average direction is necessarily parallel to the current-axis. In fact it is certain that at some parts of the stream the current sets one way, at some another.

[This is known both from the Experiment of Ch. XVII, 14a, indicating surface-motion *from* the banks, (and consequently subsurface-motion towards the banks,) and also from (the Author's preliminary) Experiments with Moore's Current-Meter, which was found to take up various "average directions" in different parts of the channel differing very markedly from parallelism with the current-axis; from the great size of the Instrument (19½" long), this was readily visible at depths not > 6 feet].

To sum up then, it may be said that—

"The 'orientation' of a Current-Meter when effected by the action of its Tail is uncertain in two ways :—

1°. There is oscillation due to the Unsteady Motion of the water.

2°. The 'Average Direction' at each point of a section may be different, and is unknown *à priori*".

3, iii. UNCERTAINTY OF POSITION.—This varies very much with the mode of use : and is worst of course when the Instrument is used with a free suspension without any vertical guys or guides. One illustration will suffice—

Use of Moore's Current-Meter. The directions supplied with the Instrument were that it was to be let down into the stream by means of a stout cord or light chain to the required depth. But when so let out it is of course at once carried down-stream by the current, so that the lowering chain no longer hangs vertically. This effect can be lessened, but by no means cured, by attaching a heavy weight to the Instrument, (thus increasing of course the obstruction caused.)

In the Author's preliminary Experiments with this Instrument, a lead weight of 10 lbs. was always slung by a short chain a little below the suspension-axis of the same : the whole mass was, however, always swept down-stream by the current, so that in the final position of relative equilibrium, the suspension-chain was always (as might be expected) in a markedly inclined position, and frequently (in consequence no doubt of transverse sub-currents) not even in a vertical plane parallel to the current-axis. But as the position-angle of the chain was unknown *à priori*, and not easily measurable (at any rate under the water), the *real position* of the Instrument was also *unknown*. Moreover, from the Unsteady Motion of the water, the Instrument kept constantly swaying both up and down, and from side to side.

But this is by no means all: the "starting action" was found to add very much to the uncertainty of position. The "starting cord" of this Instrument is necessarily a stout one, as it has to relieve the suspension-chain during the time the Meter is "in action" of the whole weight of the Instrument and its appendages, and of the Current-pressure thereon ; so that it ought in fact to be as strong as the chain itself. A stout "salmon-line" was used in these Experiments.

This line must be kept decidedly slack when the Meter is not in action. The current-pressure on so stout a line throws the "slack" into festoons between the points of attachment of the line to the chain. The sudden tug needed to gather in all this "slack", and also start the Instrument with certainty with the requisite suddenness, (to admit of accurate timing,) was found to give the Instrument a smart jerk upwards, after which it swayed about somewhat violently, so that the uncertainty of its position was much increased on first starting.

This uncertainty of position (with free suspension) is quite analagous to the uncertainty as to the depth of immersion of the Sub-Float (Ch. IX, 8, vii) of the Double-Float.

[In the case of the Moore's Current-Meter used as above with free suspension, it seemed to the Author so great as to render this mode of using the Instrument quite useless].

This fault can be almost eliminated by the use of vertical guys or guides confining the motion of the Instrument nearly to a vertical line.

3, iv. UNCERTAINTY OF GEARING.—The ordinary mode of gearing and ungearing the Instrument is by means of a light cord attached to the "retaining spring" of the

Instrument, and led up to the Observer's hand. This spring *ought* to be raised and released *quite suddenly* at definite instants recorded by a chronometer. It is impossible to raise the spring with the requisite suddenness, unless the "gearing cord" be taut at the time, (so that there shall be no slack to take in at the critical moment.) But it is impossible to preserve the cord taut during the act of lowering the Instrument into the water, with any of the ordinary modes of mounting: so that the adjustment of the "gearing cord" to the proper degree of tautness must be done *after* the Instrument is in position. This is, however, a matter of some nicety, as the "slack" of the cord has to be taken in against both the current-pressure, and friction on the rings or guides through which it passes: in a swift stream the current always exerts considerable pressure on the cord, so that in taking in the "slack" there is risk of accidentally gearing the Instrument.

[With Moore's Current-Meter (when slung from a rope or chain according to the instructions supplied with the Instrument) this risk is by no means small; it was an accident of frequent occurrence in the Author's preliminary trials. The risk was afterwards a good deal lessened (but by no means removed) by changing the "retaining spring" supplied with the Meter for a much stronger one, capable of bearing better the original tension of the cord due to the current itself].

A similar uncertainty attends the "ungearing" of the Instrument, which is not always effected at once by the simple slackening of the "gearing cord".

This uncertainty is a very serious fault, as it *throws great doubt on the value of the indications* of the Instrument.

3, v. Non-measurement of "forward velocity".—It has been explained (Ch. IV, 1) that the "forward velocity" is much the most important in a hydraulic sense. But, from what precedes (Art. 3, ii) it will be seen that in the ordinary use, (*i. e.*, when the orientation is effected by the "Tail-action") the Instrument cannot measure "forward velocity". In this use, it can only aim at measuring the—

"Average-velocity (estimated in the average direction of the Instrument's axis) of the mass of fluid particles passing",

and it follows from Art. 3, ii that this direction is unknown *à priori*, but is certainly different in different parts of the channel.

This fault is a very serious one in the utility of the Results, for it involves that—

"The Results are (at the best) velocities in unknown directions, and cannot therefore be reduced to forward velocities", ... (1).

A most important consequence follows in the application to Discharge-measurement:—

"Discharge-measurements computed from velocity-measurements with Current-Meters whose orientation depends on their Tail-action are *cæteris paribus* always over-estimated", ...(2).

This fault can only be got rid of by *fixing* the Instrument rigidly in direction (parallel to the Current-Axis): this is unfortunately a matter of much difficulty (perhaps impossible) at great depths.

3, vi. Failure near Surface.—It is obvious that the blades of the "Fan" must be completely immersed, so that the Instrument cannot measure velocity *at the surface*, nor at a depth below the surface less than the radius of the "Fan". Thus this fault increases with the size of the "Fan". It is increased also (in the absence of a bridge) by the necessity of use of a boat or pontoon, from the disturbance there-

by caused in the water near the surface: so that the action of the Instrument is uncertain at all moderate depths (within the influence of the Boat).

3, vii. A certain amount of force is required to overcome the friction of the working parts: so that with every Current-Meter there is a certain minimum velocity required to start and work the Fan; velocities less than this are not recorded at all.

3, viii. DELAY IN RAISING TO READ.—In all the ordinary patterns the Instrument has to be brought to the surface every time it is to be read. This involves a great waste of time, and a good deal of fatigue on the working Staff. In the Author's experience this is a very serious *practical* fault.

3, ix. CHOKING BY WEEDS.—In a stream bearing even *small* bits of weeds, the Fan is very liable to be partially or even wholly clogged with weeds. In such a stream great waste of time may occur from this cause, as every Experiment in which weeds have clogged the Instrument should be rejected. But the presence even occasionally of weeds throws doubt on all Experiments in such a stream; as it may readily happen that the action of the Fan might be temporarily clogged by passing weeds, without this being recognized. In fact the Instrument is quite unsuited for use in a weed-bearing stream (unless arrangements can be made for rendering the presence of the weeds visible.)

3, x. INVISIBILITY AT WORK.—The Instrument is of course quite invisible at all depths over a very few feet. This would be of little disadvantage in itself, were it not for the consequent difficulty in recognizing with certainty several of the faults above detailed, viz.—

"Uncertainty of orientation, of position, of gearing and ungearing, and lastly, choking by weeds".

3, xi. EXPENSE.—The cheapest of these Instruments known to the Author costs about £5, and they range at all prices certainly up to £12. These would not in themselves be prohibitory prices, if one or two Instruments would suffice, but some hydraulicians go so far as to recommend *simultaneous* observations at many points of a Cross-Section with a system of Meters. The expense of so large a stock would then be very great.

[*See* Mr. Clemens Herschel's suggestions at p. 126 of Amern. Socy. Civ. Engr. Trans., Vol. VII of May 1878].

3, xii. DELICACY.—The delicacy of these Instruments will always be a great objection to them. The Fan and the Recording Works are both decidedly delicate; the former is very liable to injury from accidental blows, *e. g.*, contact with the bed, impact of drift, &c., and the latter are liable to injury from rust and silt. This delicacy is a great objection on account of the difficulty and expense of repairs or renewal.

3, xiii. UNCERTAINTY OF RECORD.—The graduations of these Instruments are intended to indicate *revolutions* of the Fan, and when used in conjunction with a chronometer, the number of revolutions of the Fan per second can of course be *at once* deduced. But this is all; the velocity of the water passing can by no means be so readily inferred. In fact all modern Experiment shows that the "slip" of the screw varies not only in different Current-Meters, but even in the same Current-Meter, at different velocities, so that the *velocity of the water* passing is *by no means proportional to the number of revolutions*, but is *connected therewith by some complex relation* which is at present certainly unknown à priori*, and which can therefore

* Some Experimenters consider this relation to be linear, some parabolic, elliptic, hyperbolic and so on; so that there is little agreement.

only be determined by a laborious series of experiments separately *for each individual* Instrument (carried throughout the range of velocities at which the Instrument is to be used).

[This amounts to a great inconvenience, as no Instrument can be used with any certainty until the value of its graduations has thus been specially determined by experiment (French "tarage"): moreover, this preliminary experiment (tarage) ought undoubtedly to be done* *with the Instrument fixed to the very Boat*, and *with the very Lifting Gear to be subsequently used*, as a change of either the Boat or Lifting Gear might cause a change in the "tare" of the Instrument].

Opinion of Genl. Abbot, (Lake River Report of '70, '71, p. 621).—The opinion of Genl. H. Abbot (one of the Mississippi Experimenters) upon this point cannot fail to have great weight: he writes—

"In my opinion, founded on a somewhat close study of the subject, instruments of this class are pretty toys, which have contributed more to retard the progress of discovery in the science of river hydraulics than any other one cause. This is due principally to the fact that they register their results in a kind of cipher, to which we can by no means be sure that we possess the key.

To translate a given number of revolutions of a submerged wheel into velocity per second, and by this means to detect laws whose existence is denoted only by differences of a few tenths of feet in this velocity, is so delicate an operation, that errors in the co-efficient have usually masked the laws."

3, xiv. Constructive Faults.—These Instruments—as supplied from the makers—have often various petty faults of construction, which amount sometimes to serious practical drawbacks. Among these may be mentioned—

1°. *Use of wrong-handed screws.* The Fan is in some instruments† screwed on to the end of the rotating axle by a screw-motion opposite to that produced by the current, so that the current-action actually tends to loosen the Fan and may even entirely unscrew it.

[In the Author's practice, the Fan of a Révy Current-Meter was lost while in actual use (under water), apparently solely in consequence of the current-action unscrewing the Fan off its bearings: thus a valuable Instrument was rendered useless].

2°. *Complexity of reading dials.* In Moore's Current-Meter there are five reading dials; these are enclosed (with the recording works) in a glass tube, which is always dripping wet when the readings are made: only a small portion of each dial is visible, as they partly hide each other: alternate dials read opposite ways, (*i.e.*, some with, some against, the sun). The risk of mistake in reading is in consequence—when many readings have to be made in a long day's work—very considerable.

4. Velocity-measurement.—Supposing the "tarage" of the Instrument to have been effected (Art. 3, xiii), the question arises as to what is the nature of the velocity-measurement made. From the fact of the Instrument having to be run through a certain interval of time to furnish

* To give confidence in the Results, a certificate that this has been carried out ought always to be appended to every Report on Experiments with Current-Meters. The necessity of this has hitherto been overlooked in general.

† The Supdt. of the Roorkee Workshops reports that this was the *common construction* in the Elliott's Current-Meters formerly imported (to India) from England.

its indications, it is clear that it cannot measure the ACTUAL VELOCITY of the fluid passing it, but only the—

"Average of the velocities of the fluid particles passing within the given time",

and these velocities thus averaged will be either—

1°, Total velocities; 2°, Total horizontal velocities; 3°, Forward velocities,

according to the mode of mounting the Instrument as explained in Art. 3, v.

Thus the velocity-measurements made are in fact AVERAGE-VELOCITIES of one of the kinds above described: and, provided the mounting be such as to yield "Forward Velocities", the Result obtained (when the Instrument is run long enough to eliminate the effect of Unsteady Motion) will be of *the most practically useful kind*, viz., the AVERAGE FORWARD VELOCITY.

[Thus the Result *possible to be obtained* (under the most favorable arrangements) is more immediately useful than that given by use of (single) Floats].

5. Preliminary Experiments.—A great many preliminary trials were made by the Author both with the Roorkee Current-Meter fixed to the end of a pole, and with Moore's Current-Meter slung from a chain (as directed in the printed Instructions supplied by the maker), but the Results were not satisfactory. The Meters were tried by running them for half-minute and either one-and-a-half or five-minute intervals in succession at same depth: the Results were rarely accordant. Part of the discordance is obviously due to the Unsteady Motion of the water, but if this cannot be eliminated by running the Meter for so long a period as five minutes, there is little advantage in their use in preference to Floats. The uncertainty of gearing and ungearing the Moore's Current-Meter appeared excessive.

Out of the faults above detailed, Nos. ii to v (viz., Uncertainty of orientation, of depth, of gearing and ungearing, and non-measurement of forward velocity) appeared to the Author so serious as to *render the employment of Current-Meters* (in any way subject thereto) *simply useless.*

6. Current-Meter Lift, (Pl. LI).—The Apparatus figured in Pl. LI was designed by the Author* to secure the following advantages:—

2°. Certainty of orientation of the Meter parallel to the current-axis.

3°. Certainty of position (depth) of the Meter.

4°. Certainty of gearing and ungearing.

5°. Measurement of "forward velocity".

* The details of the Design are due to the Superintendent of the Roorkee Workshops (Mr. Angus Campbell), who kindly undertook the construction from the Author's rough directions.

[The numbering (2°—5°) corresponds to the numbering of the Faults (Nos. ii—v of Art. 8) hereby obviated].

Certain minor advantages also ensued, thus:—

i. The certainty of orientation enabled the large Tail to be dispensed* with (this being required only to preserve the orientation of the Axis), thus reducing the disturbance of the water by the Instrument, (Fault No. i).

ii. The removal of Faults Nos. ii—iv reduces the disadvantage of the Invisibility of the Instrument when at work (Fault No. x).

The LIFT consisted essentially only of a long Lift-Bar (B), to the foot (S) of which the Current-Meters were fixed, capable of freely sliding (vertically) up and down inside a Guide-piece (G) fixed on a Stand upon the platform of a Pontoon-Raft; a stout wire (*w*) connected with the Current-Meter Check Spring, working inside a groove in the Bar, served as a very efficient Gearing Apparatus.

Detailed Description. The parts of the Apparatus will be described in following order:—Guide-piece, Stand, Lift-Bar, Current-Meter Attachment, Gearing arrangement, Lower Clutch.

6a. *Guide-piece and Stand.*—The Guide-piece (G) consisted (*Fig.* 1, 6), simply of 2 sheet-iron cheek-pieces each 27" × 5" × ¼" rivetted and bolted respectively on to a back-piece and front-piece (*f*), each of 1" × ¾" bar-iron and 27" long, forming a "Guide" 27" long and of nearly 3" × ¾" sectional aperture, (inside which the Lift-Bar slid up and down.) The front-piece (*f*) was made removable by the handle *h* (*Fig.* 1), so as to admit of ready insertion or removal of the Lift-Bar. This Guide-piece was fixed (rivetted) upon the Stand at a height of 38" above the pontoon-platform. The Stand consisted of 4 angle-iron Legs splayed outwards like the 4 edges of a pyramid, rivetted at the feet to two angle-iron Foot-pieces (*Fig.* 2, 3) which thus preserved the lateral splay correct. The Foot-pieces were bolted on to the Pontoon-platform. A curved rod-iron Distance-piece D (*Fig.* 2, 3), between the front Legs of the Stand prevented the cheeks of the Guide-piece from springing open when the front-piece (*f*) was removed.

6b. *Lift-Bar.*—The Lift-Bar B (*Figs.* 1, 2, 5—8) was a 3" × ¾" bar-iron, and was provided with a toothed rack (*r*) at its back extending from the head to within a few inches of the foot (S). At the foot a socket (S) was formed for attachment of the Current-Meters. Into this rack (*r*) worked a small pinion (not visible in the drawings) fixed near the foot of the Guide-Piece by means of the handle (H) and crank-wheel (P). By this rack and pinion the Lift-Bar could be slowly and steadily raised and lowered in a vertical position. A Ratchet (R), which could be raised by the Lever (*l*, *Fig.* 1), served both to retain the Lift-Bar in any desired position, and also to prevent the sudden fall of the Lift-Bar in case of the pinion handle (H) escaping from the hands of the men working it when in the act of raising or lowering. A stout wire with a T-head (T in *Figs.* 1, 2) screwed into the head of the Lift-Bar admitted of the depression of the Lift-Bar entirely within the Guide-piece; the T-head prevented the Lift-Bar Rack escaping below the range of the pinion.

* The Moore's Current-Meter shown as in use (at M) in Pl. LI will be seen to be only 12" long: this is in consequence of the removal of its Tail, (the full length being 19".)

Three of these Lift-Bars 7′½, 10′½, and 16′ long were provided for use in different depths of water.

6c. *Graduation of Lift-Bar.*—The depth of immersion of the Current-Meter was seen by noting where the water-surface cut the Lift-Bar. To this end the Lift-Bar was marked with graduations of whole feet, the lowest mark (1 foot) being 6″ above the end of the Bar, or 12″ above the Axis of the Current-Meter (*see* next Art.). The teeth of the Rack served as minor graduations (*see Fig.* 5).

[The depth of immersion could not of course be determined very accurately (probably not nearer than ·05 of a foot), partly on account of the difficulty of seeing the water-surface distinctly through the opening (A) in the platform, partly on account of the rush of water round the Lift-Bar due to the obstruction caused by it. But this inaccuracy is inherent in the use of the Current-Meter itself, being present in all systems of mounting].

6d. *Current-Meter Attachments.*—Every Current-Meter to be used with this Lift had to be furnished with a special Foot (F in *Fig.* 1), fitting into the Socket (S) for attachment to the Lift-Bar, and also with a special Union-Link (not shown in the drawings) for connecting the long Gearing Wire (*w*) with the Gearing or Check-Spring of the Current-Meter. These Feet were designed so that in every case the Current-Meter Axis should be held *in a horizontal position, parallel to the flat side of the Lift Bar*, and at a depth of 6″ below the end of the Lift-Bar, and also so that the special Union-Link should be nearly vertical.

[It was a matter of some nicety of workmanship securing all these points at once].

Thus, when the alignment of the Pontoon-Raft was correct, the Current-Meter Axis was kept *constantly horizontal, parallel to the Current Axis, and at a known depth*, (so far as the Unsteadiness of the Pontoon-Raft would allow), thus securing the advantages Nos. 2°, 3°, 5° set forth above.

6e. *Gearing Arrangement.*—This consisted of a long Gearing Wire (*w* in *Figs.* 5, 7, 8) ¼″ thick, connected with the Current-Meter Check-Spring by the special Union-Link, worked by the Gearing Lever L, (*Figs* 1, 2, 5). The Gearing Wire was contained within a groove sunk in the front edge (*Figs.* 5, 8) of the Lift-Bar, and was retained therein by Bridges (*b* in *Fig.* 7) of sheet brass, placed across the groove, and countersunk within the Lift-Bar so as not to interfere with its passage through the Guide-piece. The upper end (*t*) was bent round (*Fig.* 1) so as to prevent the Gearing Wire falling into the groove (when the Instrument was detached).

After attachment of a Current-Meter, the weight of the Gearing Wire rested—so long as the Meter was not in action—upon the Union-Link, and through it on the Meter itself. In this state the Current-Meter was lowered into the water through the opening (A in *Figs.* 3, 4). There was only a small amount of play between the gearing attachments, so that a very slight motion (about ¼″) of the Gearing Wire sufficed to raise and depress the Current-Meter Check-Spring. To enable this to be done conveniently, a series of small holes (*o* in *Fig.* 7) were bored in the Gearing Wire. A special Gearing Lever (L) was provided fitted with a clamp (Q in *Figs.* 2, 7) for fixing it on to the Lift Bar. After the Current-Meter had been lowered into the water to the desired depth, the Gearing Lever L was screwed on to the Lift-Bar with its nose inserted into one of the holes (*o*) in the Gearing Wire at any part of the Lift-Bar that happened to be convenient (as at L in *Figs.* 1, 2). In this position a slight motion of the handle (L) sufficed to raise and depress the Gearing

Wire, and with it of course the Current-Meter Check-Spring. With a little practice this motion of gearing and ungearing could be done with great certainty.

6f. *Lower Clutch.*—To steady the vibration of the Lift-Bar when the Current-Meter was in the water, a sort of Clutch (shown in *Figs.* 1, 2, 3, 4) was provided upon the Pontoon-platform, consisting of two cheeks (C) mounted on slides which could be screwed up close together by the screw *s* worked by the crank-wheel *c*, so as to enclose and loosely grip the Lift-Bar, thus giving an additional *point d'appui* at the level of the platform.

The Clutch could be opened out by the screws *s* to the full width of the opening (A) in the platform to let the Current-Meter pass through. Thus, when the Current-Meter was in the water, the Lift-Bar was gripped throughout 27″ of its length by the long Guide-piece, and 38″ lower down by the Clutch.

7. Improved Lift.—The Apparatus as actually constructed was by no means a complete success. From want of experience, (there being no description of such an Apparatus available,) the Design itself was faulty in some points, and for a similar reason (want of a pattern, and of experience of such work) the workmanship and fitting together of the working parts were imperfect.

[The chief fault of Design was the weight and unwieldiness of the longest (16′) Lift-Bar: this made it awkward to handle, and gave rise to very unpleasant vibration in a high wind when the Bar was fully raised for reading the Current-Meter (at which time about 13′ in length projected *unsupported* above the Guide-piece). The weight of the Lift-Bar could be reduced in any future Design by making it either of smaller scantling, or else in form of a hollow (rectangular) tube: the vibration when fully lifted could be reduced by providing a taller Stand with an extra Guide-piece overhead.

The want of good fitting caused an excessive amount of friction in the working parts: so much so that it required considerable exertion on the part of two men to work the pinion used for raising and lowering the Lift-Bar. The use of brass or gun-metal (instead of iron) in the cheeks of the Guide-piece and Clutch would be a decided improvement in reducing the friction: and the substitution of a wheel and pinion instead of the simple pinion would reduce the exertion of raising and lowering. A great advantage could be gained by counterpoising the Lift-Bar, as this would reduce the exertion of raising it to that of overcoming the friction, and would enable both raising and lowering to be done with equal care, and with far greater steadiness than was possible with the uncounterpoised Bars. The use of a counterpoise would involve of course a great increase in height of the Stand, as the counterpoise-pulley would have to be placed above the extreme range of the longest Lift-Bar.

The fitting of the Gearing Wire was also by no means as good as could be wished, and involved some "humouring" in working it. This could be improved by making the Gearing Wire fit into its groove pretty closely, so as to allow no lateral play (as this causes some uncertainty in working the Gearing Lever); and the Wire itself should probably be made of brass. Partial counterpoising of the Gearing Wire would also be an improvement, as it would throw less work on the Gearing Lever.

The additions suggested would be a great improvement to the Apparatus; but great nicety of workmanship would obviously be required, (greater probably than could be expected at the present day from a Workshop in Northern India,) and the expense* would be greatly increased.

8. **Deep water Lift.**—It is clear that the use of a Lift of pattern here described is—at any rate when used from a Raft—limited to depths not exceeding 12′ or 15′, in consequence of the great height to which the Lift-Bar would rise when fully raised, (this height—above water—being at least 3′ more than the depth of water.) In deeper water therefore, some other (and less perfect) mode of mounting is unavoidable.

[In deep water the Révy method—*see* Art. 3, i above—seems probably as good as can be arranged. It has the advantage of enabling the Meter to be so fixed as to record "forward velocity", an advantage not possessed by most other modes (*e.g.*, those used in the Lake River† and Connecticut‡ Experiments).]

9. Results with the Current-Meter Lift.—From various causes, such as pressure of more important work, &c., very little use could be made of the Lift before the Experiments were brought to a close by order of Government. Enough was, however, done with it to show that, even with the somewhat imperfect Design and Workmanship unavoidable in a first attempt, it secured sufficiently the important objects (Art. 6) which it was designed to meet, and to render it certain that with the improvements suggested, and with better construction, it would have secured them very efficiently. So that *the first object of its construction may be said to have been attained.*

Moreover, when the Current-Meters were worked with this Lift, there was often a fair accordance between the Results obtained by running the Meter through half-minute and one-and-a-half or five-minute intervals in succession, leading to the belief that when worked with a favorable mounting (such as this Lift), the effects of Unsteady Motion of the water could probably be eliminated by using five-minute Runs. The amount of Experiment available was unfortunately not enough to establish this with certainty. As the "tarage" of the Instruments had not been effected, it is not thought worth while publishing the details.

* The Apparatus as actually made up cost Rs. 368 (or about £37) including the special attachments for 3 Current-Meters, but exclusive of the Pontoon-Raft.

† Lake River Report of 1869, p. 565, and Franklin Inst. Journal, Vol. LVII of 1869, p. 309.

‡ Connecticut Report of 1878, p. 307.

10. Improved Current-Meter.—Observing that a Current-Meter consists essentially of two parts, viz., the FAN which receives the impulse of the water, and the RECORDING WORKS which simply serve to record the revolutions of the Fan, and that the latter are commonly the bulkiest part of the Instrument, a *very great* improvement might obviously be effected by simply separating* the Recording Works altogether from the Fan, and placing them *above water* in any place convenient to the Observer. The connexion between them should be electrical, and should be of such a kind that every revolution of the Fan should be followed (or copied so to speak) by a revolving index above water: there would thus be no necessity for gearing or ungearing; every motion of the Fan would be "copied" by the visible index, so that every passage of the "index" past a fixed point might be timed by chronometer, (or, if too quick for the eye to follow easily, a train of wheels might be introduced to reduce the speed of the index to a rate convenient to the eye.)

The advantages gained by this alteration would be—

1°. A great reduction of the bulk of the submerged portion of the Instrument, with consequent reduced disturbance of the natural motion of the water.

4°. Removal of the uncertainties of gearing and ungearing.

7°. Reduction of the resistance to the motion of the screw by the removal of the recording works (the resistance of the recording works being exchanged for the far smaller work of making and breaking an electric contact), and consequent reduction of the "slip" of the screw, thus making the Instrument far more delicate.

8°. The saving of the delay of lifting the Instrument out of the water to read.

10°. The placing of the "copying index" in a position always visible to the Observer, thus removing many of the disadvantages of the invisibility of the Instrument.

[The numbering (1°, 4°, 7°, 8°, 10°) corresponds to the numbering of the Faults (Art. 8) hereby obviated].

The advantages in fact of this improved form appear so great, that it seems that it should supersede every other form: there is of course the little additional practical difficulty of preserving the electrical arrangements in good order, but this is believed to be a small matter.

11. Lift for Improved Meter.—To secure the advantages of certainty of orientation, certainty of position, and measurement of "forward velocity", some sort

* Instruments of the kind here described were used in the American Lake River and Connecticut Experiments, and are said to have given satisfaction; *see* Franklin Inst. Journal, Vol. VII of '69, p. 307, *et seq.*; Lake River Report of '69, p. 565, *et seq.*; and Connecticut Report of '78, p. 308.

of LIFT similar to that above described would still be necessary, but a simpler (and therefore cheaper) form would suffice, thus—

1°. The Instrument, not requiring to be lifted for reading, need only be lifted just free of the water, *i. e.*, just high enough to enable it to be detached from the Lift-Bar. The Lower Clutch might accordingly be dispensed with, and the Guide-piece might be carried down to within about 8" or 10" of the water; this would give increased steadiness to the Lift-Bar when immersed in the stream. There would thus also be less vibration in the Lift-Bar when fully lifted, as it would not be lifted so high as in the other case.

2°. The Gearing Arrangements would be unnecessary.

CHAPTER XXIV.

SILT.

Preface.—This Chapter contains an Account of the mode of Silt Collection (Art. 2—4b) and Estimation (Art. 4c—6) with Discussion of the Results (Art. 7—11). The most important Articles are Art. 8c, 9c.

1. Object of Silt-Collection.—The collections of Silt detailed in this Chapter were made with the view of tracing the connexion if any between the quantity of Silt carried and the velocity, as well as of simply estimating the Total Quantity of Silt carried by the Ganges Canal.

2. Silt-Tube, (*Fig.* 10, Pl. XXIV).—The Instrument* used for collecting samples of silt-laden water was a sheet brass Tube of 2" internal diameter, and 12' length; and therefore of capacity of 37·7 *cub. in.* per ft. of length, and about 452½ *cub. in.* in all. One end (F)—which will for shortness be called the FOOT—was provided with a moveable, close-fitting brass lid (L) working on a hinge, and furnished with a powerful spring (S). The spring was so adjusted as to retain the lid open in almost any desired position (*see* Figure), exerting then only slight pressure on it so that a very slight blow sufficed to close the lid when open: but once shut, the lid, was acted on by the spring with full power, which was sufficient to keep the lid closed and almost watertight when the Tube was full of water, even if held upright. The other end (H)—which will for shortness be called the Head—of the Tube was open.

The use of the Tube was to collect a *specimen of all strata* of the water from surface to bed at one operation. Thus on being lowered vertically into a stream, foot downwards and mouth open, the water kept rising in the Tube as it was lowered: the slight blow of contact with the bed sufficed to close the lid, thereby enclosing a specimen of all the strata of the water from the surface to the bed.

3. Field-work.—This will be described under the heads—

(*a*), Silt-Tube Handling; (*b*), Silt-Tube Emptying.

3a. *Silt-Tube, Handling.*—When a collection of silt-laden water was to be made, the Silt-Tube was first rinsed out thoroughly until quite free from dust or silt, after which the Lid at the foot was set to "half-cock" as shown in Plate; the Tube was then ready for use.

The Tube was lowered, when in use, from the side of a boat which was allowed to

* This Instrument was made over for use on these Experiments from the Roorkee Workshops, where it had lain in deposit since the closure of the Experiments projected in 1866-67 by the late Lieut.-Col. J. Dyas, R.E.

float freely down-stream along any desired line (usually one of the Sounding-Courses), the alignment being preserved in the same way as in the process of Sounding (Ch. V, 17) during the whole time the Tube was being lowered, so as to reduce the current-pressure on the Tube to a minimum.

[The first trials (No. 1, 2 of Tab. LXXXIV) were done *from a moored boat*, but the currrent-pressure in the swift stream was so great as to make the Tube almost unmanageable].

The Tube was lowered slowly, (so as to let the water rise gradually inside it,) foot downwards, lid half open and pointing down-stream, in as upright a position as possible, until the lid touched the bed: a very slight blow sufficed to close the lid, thereby enclosing a specimen of the water from surface to bed.

The spring caused the lid to close so sharply, as to be immediately felt by the man handling the Tube: he thereupon relaxed his hold, and the boat was brought to a standstill by tow ropes worked from the banks: the current immediately raised the foot of the Tube off the bed, and it was hauled in an inclined position into the boat by means of a rope attached to it about ⅔rds of the way down, (which also served to prevent the total loss of the Instrument in case of its slipping out of the hand of the man handling it,) one of the men of the party closing the open head of the Tube temporarily with his hand so as to prevent escape of the enclosed water until required.

[The handling of this long (12') Tube in a deep rapid stream was found to be such hard work, that it had always to be done by one of the European Overseers].

3b. *Silt-Tube, Emptying.*—The Tube was next held with its head over a large zinc funnel; and gently tilted so as to discharge its contents into the funnel, and thence into a large glass bottle. The lid was then opened, and the Tube well rinsed out with some of the water just discharged from it, *no fresh water being added*, until no signs of silt were visible inside it. The bottle was then closed with a glass stopper or good cork, and sent to office.

[Great care was taken to avoid spilling any of the contained water: it was not of course possible to prevent this altogether, there being frequently a trifling leakage from imperfect closure both at the head and foot.

On the other hand there was also an unavoidable trifling drainage of water from the outside of the dripping Tube into the collecting funnel. The quantity of water collected in the Tube (being 2" diameter) was, however, so great, that it is believed that these two sources of error in the *quantity* collected were quite trifling].

4. Silt Reduction.—The after operations will be described under the heads,—(*a*), Measurement of water; (*b*), Separation of silt; (*c*), Estimation of silt.

4a. WATER MEASUREMENT.—The silt-laden water contained in the large glass bottle was allowed to settle for a day or two in office, after which the whole of the upper portion was quietly decanted with a glass siphon into one or more glass measures, leaving a stratum of about 3" depth of water containing nearly all the silt. This remainder was violenty shaken up and thrown into another glass measure. In this way—there having been no loss by evaporation—the *whole volume* of water collected was measured.

[This quantity was nearly always somewhat less than the quantity collected should have been, calculated from the known depth of water at Site of collection and diameter (2") of the Silt-Tube. This is explained in Art. 9a].

4b. Silt Separation.—After measurement as above of the *quantity* of fluid collected, the large glass collecting bottle (which still contained some adherent silt) was rinsed out several times with some of the nearly clear water first drawn off into the measure last used, so that all the silt was thus collected into one measure in a small quantity of water.

The Silt was next separated from the water by passing through a "filter" of ordinary filtering paper (as used in chemical laboratories); the last portions of silt adherent to the glass vessel were removed by careful washing with a "wash-bottle": thus the whole of the Silt was finally collected on the filter paper: it was then left to dry, and after drying carefully packed away to await the next process of weighment.

[The filtration was a very tedious process, sometimes occupying nearly the whole of the working hours of one day; requiring frequent attention moreover the whole time to enable the filtration to be carried through in a day, especially in the dry hot weather; as if the "filter" once dried (as it often did, if left for the night) before the filtration was finished, its pores became clogged, after which the filtration became intolerably slow. Occasionally a "filter" broke during the filtration, thus involving a second filtration through a sound "filter"].

4c. Silt Estimation.—Before being taken into use, every "filter" was dried in a "water-bath", and then weighed (whilst hot) in a good chemical balance (by Oertling). This weight (say F) was then noted on it in pencil.

After collection of the silt upon a "filter", the filter with its included silt was again dried in a water-bath, and then weighed (whilst hot) in the same balance. Call this new weight (F+S). Then the weight of silt present was found as the difference between these two results, *i. e.*, $S = (F + S) - F$. The weighing was carried to the hundredth of a grain.

[This process of weighment* was of course very laborious. The amount of silt to be weighed was often so small (sometimes only $\frac{8}{10}$ grain—*see* Tab. LXXXIV, No. 21), that this care seemed requisite; the filter paper is so hygroscopic that it was thought necessary to do the weighments at the definite temperature given by a water-bath; this precaution was especially needed in the rainy season].

5. **Silt-Density, (σ).**—This term will be used for shortness to express the density of aggregation of the Silt in the water (not the density of the silt itself,) and it will be measured *in grains per cubic foot,* and denoted by σ. Thus—

Silt-Density, (σ) = Average Number of grains of silt in a cubic foot of water, ...(1).

The *actual* amounts of Silt and Water in each sample are shown in the Tables LXXXIV, LXXXV in *grains* and *cubic inches* respectively, so that the Silt-Density as above defined is obviously to be computed as—

$$\text{Silt-Density, } (\sigma) = 1728 \times \frac{\text{Weight of silt sample (in grains)}}{\text{Volume of water sample (in cubic inches)}}, \ldots\ldots (2).$$

* The weighment was at first kindly undertaken by Dr. Murray Thomson, Professor of Experimental Science in the Thomason C. E. College: as the work increased, it was taken up by the Author himself, the College Laboratory Instruments being made available for the work.

From the mode of collection, (the Silt-Tube stretching from the surface to the bed,) it is clear that the quantity σ thus obtained is really the—

"Average Silt-Density in the vertical of collection",........................... (8).

5a. Form of Results.—Writers are by no means agreed as to the best form of presenting the Results, nor even as to the most convenient units of measure in which to express them. The Silt-Density is in general use, but expressed in various units as shown below—

Title of Work.	Page of Original.	Year of Expert.	Units.	
			Silt.	Water.
Mississippi Report, ...	144, 145	1843-46	*n* grains	in 1 pint.
Mississippi Report, ...	187, 188	1851-53	*n* grammes	in 600 grammes.
Mississippi Report, ...	140, 141	1858	*n* grains	in 1 cub. ft.
Mississippi Report, ...	142-148	various	{ 1 part "	in *n* parts by weight. " " volume.
Royal Asiatic Socy. Journal, Vol. XX, Parts 3, 4, ...	?	1856-61	*n* parts	in 10,000 by weight.
Indus Silt Experiments, in Profl. Papers on Ind. Engng., Vol. II, Nos. LIII, LXXXIII,	20, *et seq.*	1864	1 grain	in *n* grains.
Hydraulics of Irrawaddi, ...	27—29	1877-78	{ *n* grammes *n* grammes	in 100 grammes. in 10,000 grammes.
Hydraulic Statistics, Jackson, D. A., ...	[129]	pub. 1875	{ *n* parts *n* parts	in 100,000 by bulk. in 100,000 by weight.
Roorkee Hydraulic Experts.,	869	1876-79	*n* grains	in 1 cub. ft.

The error liable to expressing the Silt *volumetrically* has been well shown in the Mississippi Report, p. 143.

The form of presenting the present Results (in grains per cub. ft.) adopted for this Work has been chosen chiefly on account of the ease with which the Total Silt-Discharge can be computed from it.

[Exception has, however, been taken* to the use of the above quantity Silt-Density as being a totally unsuitable quantity for comparison with the velocity of the current. The objector considers that the Total Silt-Discharge (per second), is the proper quantity to be used (*see* p. 4 of his pamphlet) in this comparison. It will suffice to say here that the reasoning given cannot be upheld, and has been disposed of †by Genl. Abbot].

6. Silt-Velocity, and -Discharge.—With the following notation:—

H = Depth on any vertical whose abscissa is y, *in feet*.

U = Mean Velocity past that vertical, *in feet per sec.*

σ = Average Silt-density on that vertical, *in grains per c. ft.*

* Van Nostrand's Maga., Vol. XIX for Sepr. 1878, Review of the Physics and Hydraulics of the Mississippi River, by J. B. Eads.

† In same Maga., Vol. XX, No. CXXI, for Jany. 1879.

s = Average Rate of silt passing that vertical, *in grains (per sq. ft.) per sec.*

D = Superficial Discharge past that vertical, *in sq. ft. per sec.*

S = Quantity of silt passing that vertical, *in grains (per ft. of width) per sec.*

S = Total Quantity of silt passing through the cross-section, *in lbs.* per sec.*

The quantities s, S, **S** estimated as above *in weight-units per second* are conveniently styled by the following short terms :—

s = Mean Silt-velocity past the vertical, (this corresponds to U).

S = Silt-Discharge past the vertical, (this corresponds to D).

S = Total Silt-Discharge, (this corresponds to **D**.)

Thus it will be seen that each velocity-term and symbol as U, D, **D** has its corresponding silt-term and -symbol, viz., s, S, **S**. Then it is clear that—

$$s = \sigma . U, \quad S = \sigma . D, \text{ or } \sigma . HU, \quad \ldots\ldots (4),$$

and that the Total Silt-Discharge (**S**) *should be found by the same Rules as used for computing* **D** (Ch. XIX), substituting S in place of D throughout.

When sufficient data are not available for the above (clearly the proper mode of computation), a very rough approximation to the Total Silt-Discharge may be made as follows :—

Approx. Total Silt-Discharge (**S**) = Central Silt-Density × Total Cubic Discharge $= \sigma_0 . \mathbf{D}$, ...(5).

It is clear that, in order to secure the best results, the Silt-collection and velocity-work ought—in consequence of the Unsteady Motion of the water—to be done simultaneously upon each particular vertical, and that for the last rough Result (Eq. (5)), the Silt-Collection ought to be made about the middle of the velocity-work from which **D** is computed.

7. **Sites and Verticals.**—The Silt-collections were made at four of the Experimental Sites, viz., at the Solání Embankment Main Site, Solání Twin Aqueduct Sites, and Belra Sites, and chiefly on the central verticals of those Sites.

8. **Transverse Silt-Curves,** (Tab. LXXXIII & Pl. L.)—It is obvious that Transverse Curves showing the distribution of silt through a cross-section—or shortly Transverse Silt-Curves—may be formed by plotting the quantities σ, s, as ordinates to the corresponding abscissæ (measured on the Base-Transversal): also, if these Curves be superposed upon the Mean Velocity Curve,—formed by plotting the Mean Velocities (U) past the same verticals as ordinates to the same abscissæ

* The Total Silt-Discharge (**S**) is so large a number when reckoned in grains that it is more conveniently expressed in (avoirdupois) pounds per second.

(measured on the same Base-Transversal)—the relation of the Silt to the Velocity at different parts of a Cross-Section would be exhibited.

8a. FIELD-WORK of ART. 8.—With the view of tracing out the above, a Set of Silt-collections were made on two occasions in a good many of the usual Float-Courses, once at the Solání Embankment Main Site, and once in the Solání Right Aqueduct; the whole of the collections at one Site being made as rapidly as possible one after the other. The collection in each Float-Course was stored separate, and reduced separately, so that the Average Silt-Density (σ) was thus found for each Float-Course.

[The labor—both of collection in the field, and of reduction in office—of so many separate collections was so very great, that the Experiment was not thought worth repeating. Among other difficulties it was impossible to obtain any number of large glass bottles (large enough to hold a Tube full of water), so that a great number of small bottles had to be used; this was very inconvenient. The making the Silt-Collection and Mean Velocity-Measurement *simultaneously* for each vertical was impossible with the available Staff (only two Observers): the labor of the Silt-collection was, however, so great, that the velocity-work could not be done even on the same day].

8b. TAB. LXXXIII and PL. L.—No velocity-work having been done along with these Silt-Collections, it was necessary—both for exhibiting the superposed Velocity- and Silt-Curves, and also for computing the Silt-velocities (s) and Silt-Discharges (S) past each vertical—to bring forward the Average Velocities from Series 111, and 153, 154 (combined) of velocity-work done at nearly same water-level as the silt-work.

The Results, viz., the Rod-velocities (u), and the Silt-densities (σ), and Silt-velocities (s) past each vertical are shown on Tab. LXXXIII and Pl. L. The Table shows also the corresponding Cubic Discharge (D), Total Silt-Discharge (S), (computed as in Art. 6,) and resulting Mean Silt-Density ($\bar{\sigma}$), and Mean Silt-velocity ($\bar{s}$).

8c. DISCUSSION.—The Diagrams (Pl. L) show the Results at a glance. It cannot be said that there is any correspondence between either of the curves of Silt (viz., of σ, s) and that of Velocity (u). There is a marked correspondence between the two Silt-Curves (those of σ, s); but no Conclusions can be drawn from this, because this correspondence is due simply to the fact that the values s are the product of the two factors σ, u, whereof u varies but little right across the channel, whereas σ varies greatly and abruptly, so that the Curve of s necessarily follows that of σ to a great extent. The only Conclusion possible is that as far as the available data (only two Curves) go,—

> "There is no obvious connexion between the Velocity and Silt-Density at different parts of a Site",..(6).

A probable explanation of this Result seems to be that on both the days the Silt-Collections were made, the water was *not nearly fully charged*, and that the great and irregular variations of Silt-Density from one part of the channel to another are due to the irregular motion of the water, whereby the Silt-Density in any one vertical is liable to vary a good deal from instant to instant. That such variations do occur from instant to instant at any one spot can be readily seen by the eye, rapid

variations in its cloudiness being quite obvious (at any one spot) whenever the water is moderately turbid.

It would seem then that just as the Unsteady Motion of the water renders it necessary to obtain Average Velocity Results for intercomparison, so there is probably also an Unsteadiness of Silt-Density, in consequence of which Average Silt-Densities should be sought for comparison with each other and with Average Velocities.

[If this be really the case, it will be little use attempting to pursue the subject, as the labor of the Silt-Collections will probably practically prevent Averages being obtained].

9. Central Silt-Densities, (σ_0).—By far the greater number of the Silt-Collections were made on the Central Vertical only, at following Sites :—

7 at the Solaní Left Aqueduct, 17 at the Soláni Right Aqueduct,
49 at the Belra Site.

The details are given in Tab. LXXXIV, LXXXV. The Central Silt-Densities (σ_0) and Approximate Total Silt-Discharges (S) were found as explained in Art. 5, 6, (*see* Result (5)).

9a. Solani Sites, (Tab. LXXXIV*).—This Table shows both the actual quantity of water collected in the Silt-Tube, and also the quantity that *might have been expected*, computed by multiplying the cross-section area of the† Tube by its immersed length (*assumed* to be the same as the Average Depth on the vertical). The difference between these—(always Loss except in No. 86)—gives an idea of the difficulty of handling the Silt-Tube.

[The collections Nos. 1, 2 were made from a moored boat (Art. 8a); the rest were all made with the boat freely floating. The Loss in the first mode will be seen to be very large, whilst in the second mode it was usually small : the improvement is very marked. Observe that some "Loss" is to be expected, because the Silt-Tube was closed by the spring when it first touched the bed, *i. e.*, before the mouth of the Tube actually reached the bed. No deduction has been made for this in computing the expected quantity].

The "expected quantity" on the rough bed of the Embankment Main Site is necessarily somewhat conjectural, being computed from the Average Depth (H) in the Sounding-Course, whereas the Silt-Tube was closed by its spring touching the actual bed sometimes in a rise, sometimes in a hollow; the actual collection might even exceed the "expected quantity" in the latter case (as indeed happened once, *see* No. 86).

From want of sufficient Staff, the Silt-Collections at these Sites could not be done in general in connexion with the velocity-work of Discharge-measurements. In calculating the Total Silt-Discharges (S), it has accordingly been necessary to obtain the Cubic Discharges (D) by interpolation from Abstr. Tab. 14, 15.

9b. Belra Site, (Tab. LXXXV).—A Silt-collection was made‡ on the central

* This Table also contains details for other verticals (Art. 8a) as well as for the central vertical.

† Sometimes two Tubefuls were collected : in this case the "expected quantity" has of course been computed as for two Tubes.

‡ Most of these Silt-collections were made by Mr. G. Henry, a few only in January by Sergt. Porters.

vertical at this Site once a day (with one exception, viz., on 10-1-'79), along with (*i.e.*, just before or after) one of the Discharge-measurements, the Staff having been specially increased, (*see* Ch. XXI, 9d.)

The Total Silt-Discharges (**S**) at this Site have accordingly been computed from better data than the preceding, the two factors (σ_0, **D**) having been in each case *obtained in concert.*

The data and Results of the Silt-Collections have also been arranged (Tab. LXXXV) in Series corresponding *Set by Set* with those of the velocity-work, (Ser. 201—206, Tab. L, LI).

9c. DISCUSSION.—The most striking feature of both Tab. LXXXIV, LXXXV, but especially of Tab. LXXXV, is the extreme variability of the Silt-Density and -Discharge with trifling variations of depth and velocity.

The only Conclusion possible seems to be that—

"The Silt-Density (and therefore also Silt-Discharge) do not appear to depend sensibly on either the depth or velocity", ...(7),

and if they do so depend, then it is clear that there must be other far more efficient causes at work capable of wholly masking such dependence. It is well known that a current is *capable of carrying* a quantity of Silt increasing with increase of velocity : but it by no means follows that increase of velocity is necessarily accompanied with increase or decrease of Silt-Density, as for this to take place involves the existence of a very loose bed always ready to give up Silt as the velocity increases.

Now the Ganges Canal is fed from the Ganges at a point where the water varies from great clearness to great turbidity at different seasons, and also receives the drainage of several hill torrents—(at various points from the Head-works down to Dhanaurí)—which run only after heavy rain, and are then always heavily silt-laden.

But the depth and quantity of water passing through any Reach of the Canal are regulated chiefly according to the requirements for irrigation, without any reference to the amount of Silt in the water admitted into the Canal, either from the Ganges or from the drainage-inlets. It seems probable then that—

"The Silt-Density and -Discharge in the Ganges Canal depend chiefly on the quantity of Silt present in the Supply* admitted into the Canal ",.................(8).

The above is confirmed by examining the state of the Silt-Density at the Solání Aqueduct in Tab. LXXXIV ; being there arranged *by order of date*, it is easy to examine the seasonal change of the Silt-Density, and it is at once seen that at this Site the Silt-Density is least in the cold weather months, October to March, (when the Ganges is low,) and greatest in the height of the rainy season, August and September (when the Ganges is in flood).

[A single exception to this, viz., a very large Silt-Density in the month of January (No. 24 of Table) is probably due to a heavy flood from the cold weather rains].

The Experiments at Belra, lasting only from January to March, do not of course suffice to show any seasonal change. The variations at this Site are, however, extreme, and appear to be only accountable by freshets of drainage water.

10. Silt at different Sites.—The amount of Silt contained in the Canal water is extremely different at different points of its length, as is

* *i.e.*, Supply from all sources, both from the River Ganges, and from the hill torrents.

evident from the following Table* of Silt-Densities from 12 samples of water *taken on same day*—

CANAL.	Main Canal.							Etáwah Branch.		Cawnpore Division.			Date.
Mileage from Head-works, (of Main Canal or Branch), ..	½	19½	48	62	101½	156	161	½	48	48½	108½	169½	26-5-'64
Silt-Density (σ), *in grs. per c. ft.*, ...	24·9	44·1	29·9	42·8	45·5	169·5	315·9	150·1	226·1	246·7	524·6	435·5	

It would seem from the above that—

"The Silt-Density increases pretty steadily with increase of distance from the Head-works both in the Main Canal and Branches",..................................(9).

The Bed-slope of the Canal decreases with the distance from the Head-works, so that the Canal should deposit silt (in consequence of the decrease of velocity) rather than take up more. The increase of Silt can only be ascribed either to increased friability of the soil in the lower Reaches, or else to the state of turbidity of the drainage-water admitted into the Canal on the day of the Experiment, increasing with the distance from the Head-works.

11. Silt. QUALITY.—The samples of Silt taken near Roorkee appear† to be—

"Chiefly micaceous sand, with a little clay (probably of recent origin, derived from disintegration of felspathic rocks), and traces of iron and lime".

12. **General Conclusions.**—In face of these Conclusions, it seems that a Canal subject to great variation in the amount of Silt admitted into it is unsuitable for Experiment on the connexion between silt and velocity.

This is a disappointing Conclusion, as the amount of labor expended on the 90 Silt-Collections (and Reductions) here reported was very great.

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* from information furnished by Dr. Murray Thomson, who made the Silt-Estimation himself from collections supplied to him by the Canal Staff in 1864.

† from report of Dr. Murray Thomson, to whom several samples were submitted.

CHAPTER XXV.

EVAPORATION.

Preface.—This Chapter contains an Account of the mode of measurement of the Evaporation from the Canal-surface (Art. 2—4b), and of the temperature of the water (Art. 5—5d) with Discussion of Results (Art. 6—10). The most important Articles are the Discussion, Art. 8—10.

1. **Evaporation from Canal.**—An attempt was made to measure the Evaporation *from the surface of the Canal* itself near Roorkee at end of 1876, and was carried on continuously, with occasional interruptions, until the close of the Field-work in April 1879.

The question is one of considerable practical interest, as from the immense surface of the Main Canal, of its Branches, and Distributaries, exposed to the long continued dry weather, and to the hot winds of Upper India, it was supposed that the Evaporation would be very large.

2. **Conditions for Evapometer.**—The heating effects both of the sun and of the hot winds on any vessel exposed to them are so great, that it seemed essential that the vessel to be used as an EVAPOMETER should be floating in the Canal water, so as to be *kept at the temperature of the body of the Canal* by the continuous flow of the water past it.

In consequence also of the ever-varying level of the water in the Canal, it was absolutely necessary that the Evapometer should be *freely* floating in such a way as to be quite free to rise and fall with the changes of the Canal.

[These Experiments were made at the suggestion of, and were initiated by, Dr. Murray Thomson, to whom also the design of the Evapometer described below is due. They were simply carried out by the Hydraulic Experiments' Staff from instructions supplied by him. Similar Experiments had been tried by him some years before; but want of boats had prevented the Evapometer being properly moored (Art. 6a) in the free channel].

3. **Evapometer, (Pl. XXIV, 11).**—The complete Instrument consisted of the following parts, which will be described in order :—

(*a*), Evapometer-Pan ; (*b*), Splash-board ; (*c*), Float-Frame.

3a. EVAPOMETER-PAN.—This consisted essentially of a square pan (or open box) of stout sheet zinc 12" × 12" in plan, and 9"* deep, stiffened by a stout wire

* The depth has been incorrectly shown (both drawn and figured) as 12" in the Elevation in Pl. XXIV, 11.

beading running all round the upper edge; and with four stout iron eyes (k) at the four upper corners for hooking on the lifting chain.

Two Strips of thin sheet zinc (s) with scales (similar to those of a common levelling staff) painted on them were fixed inside the Pan at the middle of two opposite sides. The zeros of these scales were on the flat bottom of the pan, so that the depth of water in the Pan could be read directly on the scales.

[These scales were at first divided to $\frac{1}{100}$ of a foot; these were afterwards changed for scales divided to $\frac{1}{10}$ of an inch: the published Results are all reduced to inches].

3b. SPLASH-BOARD.—When floating in a rapid stream, the Pan (made up as above) was found to dash about so much, (in consequence of the necessity of mooring it,) that it became absolutely necessary to provide a *raised freeboard* for it, partly to prevent its actually ducking under, partly to prevent spray being blown into it by the wind. This was done by attaching a sort of square funnel 10" deep above the square pan, exposing an opening of 30" × 30" at the top, and sloping down on all four sides to the upper edge of the Pan, (*see* sketch.) This arrangement was found a sufficient protection from spray driven by the wind.

3c. FLOAT-FRAME.—To enable the Apparatus to *float* in the water, a stout wooden frame (like a picture-frame) was made up of four wooden bars each 30" long by 3" × 3" scantling, lapped across one another so as to leave a central opening a little more than 12" × 12" into which the Evapometer fitted easily. The buoyancy of this frame was increased by the addition of four closed air cylinders (C) of sheet-zinc, each 12" long by 3" diameter, which were fixed underneath the wooden frame as shown in sketch. A stout iron ring (R) was fixed at one corner of the frame for the attachment of the mooring chain.

4. Use of Evapometer.—When about to be used, the Pan was first thoroughly cleaned inside; and then, after being placed on as level a spot as could be found, water (taken from the Canal itself) was poured in to about 6" depth. The actual depth of water was then read *on both scales by the same Observer* as accurately as possible: water was added or taken away until the mean reading on the two scales was *very nearly* 6".

The Pan was then lifted with a light pair of shears and tackle, and carefully lowered down into its final position inside the Float-Frame, which had been previously floated in the water, and brought close up on purpose. The lifting tackle being then detached, the apparatus was let go in the stream, and finally moored by a long rope or chain. The water-surface inside the Pan when thus floating in position was about 2" below* the free surface of the water in which it floated.

It was then left to itself in the Canal a sufficient time (about a week) to admit of a measurable amount of loss by evaporation taking place. After this, at any time that was convenient, the Apparatus was brought to bank, and the Pan was carefully lifted out of the Float-Frame with the help of the shears, and deposited again on as level a spot as could be found; the actual depth of water remaining in the Pan was then read again as accurately as possible *on both scales by the same Observer*.

The difference between the Means of the scale-depths in the Pan at the beginning and end of the Experiment was considered to be the—

"Apparent Loss by Evaporation in the period (diminished by rainfall or dew)".

* This was of course an objection: the two surfaces should have been on same level.

[*Introduction of Sand.*—A small amount of dust or sand was often found in the Pan on withdrawal from the water, having been blown in apparently by the frequent high winds. The presence of this foreign matter would of course slightly raise the water-level to be measured at the end of the Experiment, and *pro tanto* diminish the apparent Loss by Evaporation].

4a. Scale-readings inaccurate.—The transparency of the water, and the irregular capillary action between the water and the surfaces of the scales prevented a very clear definition of the plane of the water-surface upon the scales. This want of good definition, coupled with the considerable distance of the reader's eye (about 25") from the part of the scale in question, prevented the scale-readings from being as accurate as could be desired. To prevent introduction of personal equation effects, (which are likely to be comparatively large in this sort of observation), it was made a rule that the readings at beginning and end of the Experiment should be *taken by the same Observer.*

On the whole, it is considered that the readings cannot be depended much closer that the $\frac{1}{10}$th of an inch. To prevent the chance of inaccuracy in the readings masking the quantity sought, it was thought desirable to leave the Pan in the Canal for about a week, to admit of the accumulation of the Loss by Evaporation to about $\frac{1}{2}$".

[It is not very obvious how the accuracy of *direct* scale-readings could have been increased without having the Pan made partly of glass with the scales engraved thereon, so that the eye could be applied directly to the scales as with a graduated glass measure: or else by the use of some sort of cathetometer applied from the inside].

4b. *Imperfect levelment.*—In order that the scale-readings should really show the depth of water in the Pan, the bottom of the Pan should of course have been truly level at the time of reading. The means adopted of securing this—simply placing the Pan on as level a spot as could be found—was of course imperfect. It would have been a decided improvement to have provided a level masonry bed on which to place the Pan at time of reading. It was thought, however, that the provision of the two scales on opposite sides of the Pan, supplied a sufficient correction for the slight inaccuracy of levelment.

4c. *Improved Measurement.*—It was suggested to the Author that the amount of water at beginning and end of the Experiment should be ascertained either by weighment, or by measurement in a special graduated vessel.

4c, i. *Weighment.*—The chief objection to weighment was its expense: involving the provision of a *good* balance weighing up to 40 lbs. (the water alone weighing 31½ lbs.), and also of a weighing house to enable the weighment to be done in a high wind; but it has also uncertainties and inconveniences of its own. Small uncertainties are introduced by the unknown weight of the film of water adherent to all parts of the Pan just before insertion (the remains of washing it out), and adherent to the immersed parts outside just after withdrawal.

On withdrawal of the Pan from the water, there was frequently a small quantity of dust or sand at the bottom, (sometimes amounting to perhaps 1 oz.,) blown in of course by high winds; the separation and separate weighment of this would have been troublesome.

4c, ii. *Measurement.*—This seemed a more hopeful method. A special measuring vessel was prepared of cast-zinc of nearly half a cubic foot capacity, tapering up near the top to a vertical cylindric tube of 14·4 sq. in. sectional area, so that a fall of $\frac{1}{10}$ inch in the Evapometer Pan (which was 144 sq. in. in plan) corresponded to a fall of 1 inch in this tube. The water-level in this Tube was to be read on the graduated stem of a Float, like that of a rain-gauge. There seems no doubt that this arrangement would have been pretty successful (although it has some objections of its own); it was not made up, however, until so late a period of the Experiments that it was thought better to continue the existing mode of scale-reading (so as to preserve uniformity in the mode of reading throughout the whole period): and it was accordingly never introduced.

5. Temperature.—The maximum and minimum temperature of the water itself during the period of each Experiment are elements of considerable interest, as they of course greatly modify the Evaporation. Unfortunately the attempt to measure it was found to involve considerable risk to the thermometers. In consequence of the varying state of the Canal water-level, a fixed thermometer could have been of little use, unless deeply immersed, (as it would be otherwise liable to be left high and dry by a fall of the water): on the whole it seemed best to attach the thermometer to the Evapometer itself, so as to rise and fall with it.

5a. *First trial.*—The first attempt was made at the end of 1876, *i.e.*, soon after the Evaporation Experiments were started. A maximum thermometer was simply laid flat inside the Evapometer-Pan, intended to be read on withdrawal of the Pan. Unfortunately the Evapometer-Pan got loose (by some accident) soon after the Experiment was started, and was lost for some weeks; and when found (30 miles lower down, after passing over four Falls), the thermometer was of course missing. In consequence of the discouragement resulting from this mishap, no further systematic temperature-measurement was tried till 1878.

5b. *Second trial.*—In April 1878 another attempt was made. A maximum thermometer was attached (in such a way that it could be removed for reading) to the side (*see* T in Pl. XXIV, 11) of the wooden frame-work of the Float-Frame, so as to be always about 1″ below the water-surface when in use.

This thermometer was read twice in each Experiment, so as to furnish—

1°. The actual temperature of the water at beginning of the Experiment.

2°. The maximum temperature of the water during the Experiment.

This thermometer also was lost after this arrangement had been started some weeks: altogether only four complete observations were obtained from it (*see* Tab. LXXXVI).

5c. Precautions necessary.—The reading of the thermometer as above (*i.e.*, for water-temperature) requires some special precaution, especially in the hot weather, in consequence of the great excess of temperature of the air (in which the readings take place) over the water.

1°. To read the *actual temperature*, the thermometer must be kept plunged for some time in the water to acquire the water temperature. On removal from

the water it must be read *immediately;* for if there is the least dry wind moving, the evaporation from the dripping bulb is so rapid *at first* as to depress the mercury rapidly (sometimes 2° in the mere art of lifting from the water to the eye): when this effect is past, the mercury rises rapidly in consequence of the excess of heat of the air.

2°. The same difficulty might obviously occur as to reading the *minimum temperature*, if the actual temperature of the water was close to the minimum at the time of reading: as on first lifting out of the water the minimum index would be rapidly depressed from the same cause as above.

3°. In "setting" the maximum thermometer similar precautions must be observed: the thermometer must first be plunged in the water long enough to acquire the water-temperature. It may then be removed, and is conveniently "set" in the open air whilst the mercury is in the *temporary state of depression* above noticed.

It might be supposed that some of these inconveniences might be got rid of by reading the thermometers under the water: this is unfortunately a difficult thing to do from a boat which is moored in a rapid current.

5d. *Thermometric Results.*—The measurements of the water-temperature, though few in number, are very valuable in clearly showing the *unusual coldness of the water*, the maximum temperature registered being 75½° at a time when the maximum in the air was* probably 113·5° in the shade and 172° in the sun.

6. **Experimental Sites.**—The Evaporation Experiments were necessarily conducted at Sites easily accessible to the Experiments' Staff in the ordinary course of their regular work. They were accordingly made at the following Sites:—

Soláni Aqueduct, from Novr. '76 to Decr. '78, and in April '79.
Kamhera Site, from Jany. to March '79.

6a. SOLANI AQUEDUCT.—In a navigable Canal—like the Main Ganges Canal—and in the neighborhood of a town (as Roorkee), it is by no means easy to find a Site, at which such an Instrument as an Evapometer could be safely left for days together, in a position free from risk of being injured by passing craft, or of being wilfully tampered with.

In the present case the Instrument was moored to the tail of the Central Pier of the Soláni Aqueduct (Pl. II, 3) by a rope or chain long enough (about 20' long) to let the Pan swim freely clear of the backwater of the Pier itself: the weight of the chain was partly supported on a special wooden Float near the Pan, so as to prevent its tending to drag the Pan under.

[This position was a very good one: the Pier itself protected the Instrument almost entirely from risk of injury from passing craft, and being at mid-channel it could not easily be tampered with by passers by on the bank. In fact, in consequence of the swiftness of the current, it could only have been reached (without the aid of

* These were the figures at the Thomason C. E. College Observatory, about 1 mile distant.

a boat) by an expert swimmer. On one occasion a bullet hole was found in it, and this was the only occasion on which it was *known to have been tampered with*].

6b. *Paucity of Results.*—Notwithstanding that the Experiment was kept up as far as possible continuously, only 27 Results were obtained (*see* Tab. LXXXVI) during the two years' (Novr. 76 to end of 1878) work at the Solání Aqueduct. This is accounted for as follows :—

In the first place there were three breaks within the period, viz.—

1°. Two months (Janry. and Febry. '77) stoppage in consequence of the loss of the Instrument from its moorings in December '76.

2°. Four months stoppage in the rainy season of 1877.

3°. Four months stoppage in the rainy season of 1878.

This reduces the period of actual work (to end of 1878 from 25 months to 15 months, or about 456 days; on reference to the Table, however, it will be seen that the Instrument was apparently actually in the water only 230 days; or about half the total possible time.

The remaining half of the time was simply lost in abortive Experiment. The chief causes of loss in this way were rain and wind. Thus, the area of the mouth of the funnel sloped splash-board being 6½ times that of the Pan, the slightest shower completely masked the whole Evaporation-Loss, and a moderate shower would sink the Instrument outright. Such slight showers were sometimes of very frequent occurrence. A high wind would also sometimes submerge the Instrument. In this way the Results of very many Experiments were lost.

[An attempt was made in April 1878 to prevent the total loss of the Results caused by *slight* showers by placing a Rain-Gauge on the Central Pier of the Aqueduct close to the moorings of the Evapometer, so as to obtain a measure of the actual rainfall close by the latter. The Rain-Gauge was the common one* of Fleming's pattern, fitted with an *additional* funnel-receiver of four times the area of the usual funnel-mouth of the Instrument, so as to be four times as delicate. The Rain-Gauge did not prove of much use after all: slight rainfalls were registered in it on only two occasions, (Nos. 20 and 25 of Tab. LXXXVI); these have been *added* as "corrections" to the actual Loss in the Evapometer. All other Results at this Site known to be affected by rain *have been simply rejected* as worthless.

This loss from rain could not have been wholly prevented except by removing the Instrument into shelter whenever rain was imminent. This would have involved the presence of at least two men always on the spot: the situation (at the Solání Aqueduct) however would not conveniently admit of this, and it was not thought worth while to incur the expense].

6c. Kamhera Site.—The Instrument was simply loosely moored at mid-channel of the Anupshahr Branch Canal, a litte way below the Kamhera Discharge Site, (Pl. VI). This Canal not being open for navigation at the time, the Instrument was free from risk from passing craft: the camp of the petty Establishment moreover was formed at the Discharge Site, so that there was very little risk of the Instrument being disturbed by passers by.

[This Site was in fact chosen in preference to the Belra and Jaolí Sites, at which work was going on at same time (Ch. XXI, 9), in consequence of its freedom from

* largely used in the N. W. P. of India under the name of the "Revenue Board pattern".

risk to the Instrument. The presence of the petty Establishment camp on the spot immensely increased the number of successful Experiments: for whenever rain was imminent the Pan was *removed under shelter*, and thus the principal source of loss of observations was removed].

7. Results, Tabulation, (Tab. LXXXVI).—The Results are shown in the Table: the headings of the Columns are in general sufficiently explanatory.

In the case of the Experiments at the Soláni Aqueduct, certain meteorological data have been given (obtained from the Thomason C. E. College Meteorological Observatory, about 1 mile distant), as being likely to throw some light on the causes of increased or decreased Evaporation. These are—

1°. *Mean Temperature*, obtained as the arithmetic mean of the maximum and minimum in the shade.

2°. *Mean Humidity*, obtained as the arithmetic mean of the "humidity" computed from the 10 A.M. and 4 P.M. observations.

3°. *Wind, Prevailing Direction*, obtained simply by inspection of the directions recorded at 10 A.M. and 4 P.M.: this is of course a very rough process.

4°. *Wind Velocity*, (in miles per day): obtained by dividing the total mileage of wind during each experimental period by the number of days of the period.

The above are of course very rough data: but more accurate results could only have been got with great labor, and the increased accuracy would have been of no practical use.

8. Discussion.—The most striking feature in these Results is the *very small amount* of the daily Evaporation, together with the extraordinary fact that it does not appear to be very much greater in the hottest months of the year (May and June) when a scorching hot wind is blowing during great part of the day.

8a. *No accession of water.*—The amount of Evaporation is so small as to throw *at first sight* some doubt on the sufficient correctness of the mode of use of the Evapometer. The only obvious source of undue reduction of the measured Loss by Evaporation is the possible accidental *introduction of water from without* into the Evapometer. This might occur through any of the following causes:—

1°, Leakage; 2°, Spray driven by the wind; 3°, Rainfall; 4°, Heavy Dew; 5°, Tampering with the Instrument.

The evidence against their occurrence is chiefly negative:—

1°. *Leakage.* It may be pretty confidently asserted that Leakage did not occur; as the presence of any leak, however small, would certainly have led to the Pan filling and sinking outright in the number of days covered by each Experiment.

2°. *Spray driven by the wind.* This seems a likely cause of introduction of water from without. The raised funnel-shaped edges of the Pan were

intended to protect it from this, but it is by no means certain that they did do so sufficiently. In a high wind the Pan was often violently agitated (in consequence of being moored in a swift stream); and it is quite possible that spray was occasionally blown in. The best evidence against this occurrence (or in favor of the sufficient protection of the Pan) is that—excluding cases where rain was known to have fallen—only one case occurred (No. 13) of more water being found in the Pan on removal from the Canal than was originally placed in. If the protection from spray was insufficient, it would seem that this case should have occurred frequently.

3°. *Rainfall.* It has been explained (Art. 6b) that a very small shower would suffice to mask the Evaporation-Loss: but, from the way in which this difficulty was met (*see* Art. 6b,c), it seems certain that rainfall does not sensibly affect the published Results.

4°. *Heavy Dew.* The mode of experiment obviously only really shows the excess of evaporation over condensation of dew: and it is just possible that the latter might even exceed the former, causing thereby an *apparent gain* in the Evapometer-Pan. Only one such case of *apparent gain* in the Pan (No. 13 of Table) occurred during the Experiments: as this occurred in a season of heavy dew at night, it is quite probable that this abnormal Result is a correct one.

[This Result (apparent gain in the Evapometer) is by no means unknown in the (colder climate) of England, *see* Civ. Engr. Inst. Procgs., Vol. XLV of 1876, p. 27].

5°. *Tampering.* The situation of the Evapometer at both Sites has been explained (Art. 6b,c) to have been such as to render any tampering with it decidedly difficult: so that this also must be rejected.

On the whole then the Results may be looked on as ***not unduly reduced by accidental introduction of water from without***, and must therefore be accepted as substantially correct.

8b. OTHER RESULTS.—The Results are certainly very small (in no case exceeding ·37″ per day) when compared with the evaporation recorded for other places in India. But the fact is that in most of these cases the evaporation recorded is that from a *small* Evapometer ***on dry land***, the water in which is therefore *liable to be superheated.*

Tank Evaporation. The Evaporation from (an Evapometer floating on) a large still water-surface is known to be much less than from a small vessel on land; as witness the following* Results:—

At Red Hill, Madras.

Mean daily evaporation April to August,	·374″ from tank,	·469″ on the land.
Mean temperature of water in same period,	81° in tank,	83° on land.

River Evaporation. Again, from the American Lake River Experts., (Report of '70-'71, pp. 570—573,) it appears that the Evaporation from (an Evapometer floating

* by Mr. Ludlow, quoted in Annales des Ponts et Chaussées, Vol. XVIII, 1869, pp. 225—237, and Jackson's Indian Meteorological Statistics, 1875, pp. (41), (42).

on) the surface of a River is usually much less than from a small vessel on land, as seen in Abstract below—

EVAPORATION.	St. Clair River.	Niagara River.
From river < from land,	47 cases.	90 cases.
From river > from land,	5 cases, (3 at night,) all small.	20 cases, (17 at night,) all small.

The few cases in which the river evaporation was the larger are all small in amount, (so that a small error would largely affect them.) It is curious that these cases occurred chiefly at night; the probable explanation of this is that the land is liable to more rapid cooling at night than the river.

8c. *Evapometer not superheated.*—To test the efficiency of the mode of floatation of the Pan in preventing undue heating of the water within it, the following temperatures were observed *at a very hot season* :—

Date 22-5-'77, *Time* 2-30 P.M.

At Tail of Soláni Aqueduct, { Temperature of water in Evapometer,.................. 66°.
„ „ in Canal,.......................... 65°.

At the Thomason C. E. College Meteorological Observatory, [about 1 mile distant], { Temperature of the air, in shade, ...105°.
Max. „ „ „ ...106°.
Max. „ „ in the sun, 165°.

This shows sufficiently that the water in the Pan was *not unduly heated.*

8d. CONCLUSIONS.—The relative smallness of the Evaporation from the Ganges Canal, (near Roorkee,) which is especially remarkable in the hot weather months, is most probably then *due to the unusual coldness of the water* (65° to 75° in June, *see* Tab. LXXXVI).

It must be remembered that the River Ganges which supplies the Canal is a snow-fed river, and that the Canal-Head is just at the spot where the river leaves the mountains. The two Experimental Sites were distant from the Canal-Head as follows:—

Soláni Aqueduct, Tail of Central Pier, about 18 miles.
Kamhera Discharge Site, about 52½ miles.

The water at the upper (Soláni Aqueduct) Site is actually about its coldest* during the hottest months of the year, the Ganges River being in those months greatly swollen with *freshly melted* snow.

From all the Results it would seem that—

"The Evaporation from the Canal-surface near Roorkee averages about $\frac{1}{10}$" daily (out of the rainy Season)," ....(1).

9. **Dependence on weather.**—The dependence of the Evaporation on the meteorological elements quoted, viz.—

1°, Mean Temperature; 2°, Humidity; 3°, Wind,

* It is in these months actually the coolest water obtainable in Roorkee without use of ice.

is by no means obvious. On examining the 28 cases fully reported for the Soláni Aqueduct (Nos. 1—28, Tab. LXXXVI), it seems, however, clear that a combination of high temperature, low humidity, and high wind is (as might be expected) usually accompanied with high evaporation, and *vice versâ*.

10. Total Evaporation from Canal.—It becomes an interesting question to estimate the Total Evaporation from the whole length of the Canal and of its Branches and Distributaries, for comparison with the Total Supply admitted into the Canal. The whole Area of water-surface exposed to Evaporation has been obtained from following data :—

CANAL.	Total Length [in miles].	Surface-breadth [in feet].	Area [in sq. ft].	Total Area [in sq. ft]
Main Canal, ...	350	190 to 60	281,000,000	487,080,000
Branch Canals, ...	300	70 to 30	79,200,000	
Major Distributaries, ...	3000	10	158,400,000	
Minor Distributaries, ...	700	5	18,480,000	

Assuming from Art. 8d the rate of Evaporation to be about $\frac{1}{10}''$ or $\frac{1}{120}$ of a foot per day, the Total Evaporation would be 4,059,000 c. ft. per day, or 47 c. ft. per second, which is about $\frac{1}{149}$th part of the Full Supply (7000 c. ft. per sec.) into the Canal : thus—

"The Evaporation-Loss is about $\frac{1}{150}$ part of the Full Supply of the Canal",...(2), or in other words—

"About 10 minutes' Full Supply of the Canal is lost daily in Evaporation",... (2a).

CHAPTER XXVI.

SUMMARY.

Summary of Results.—A pretty full Summary of the Results obtained in this Work is given below, so that they may be seen in a collected form *without the necessity of reading the details* either of the Experiments or of the Arguments on which they are based. The references to Articles (black letter figures, as **14**, *on left* of page) and Results (numbered, as (7), *on right* of page) will enable consultation of details to be made when desired.

ABSTRACT OF RESULTS.

Chap I.—Introduction.

1. "Existing Modes of Discharge-Measurement of large bodies of water are of doubtful accuracy".

3. "New Experiment is wanted *on large bodies of water in motion under simple Conditions, e.g.*, in large regular Canals".

7. "A Site—to be very favorable for Experiment—should be situate in a straight uniform Reach of great length, *i.e.*, with uniform Banks, uniform Bed, and uniform Bed-slope for a great distance above and below", ... (12).

—"and, to be suitable at all for Experiment, must not be situate in a marked hollow in the Bed-slope", ... (12*a*).

Chap. IV.—Velocity-Measurement.

1. "Velocity in Hydraulics usually means 'Forward Velocity', *i.e.*, the *resolved part* of the actual velocity taken parallel to the current-axis".

4a. "Mean Velocity-measurements of all kinds are not sensibly affected by Errors in estimating Depth, or Breadth, but only by Errors in the primary Velocity-measurements on which they depend", ... (8*a*,*b*).

6. About the "Essentials of a Float".

7b. "Neither obliquity nor crookedness of Float-path necessarily interferes with the proper use of Floats", ... (5*a*,*b*).

7c,d. "Floats measure the Average of the Forward Velocities of the fluid particles successively displaced along the Float-path", ... (6)—(9*b*).

9. "Floats can be used with advantage only at a 'Favorable Site', (as in Ch. I, (7))", ... (12).

3 D

10. "and not near irregular banks, nor very close to any banks however regular".

11. "Floats have many advantages over Fixed Instruments, viz.—
1°, interfere little with the natural motion of the water;
2°, measure velocity directly; 3°, can be used in streams of any size;
4°, are little affected by silt or weeds; 5°, measure forward velocity;
6°, can be made and repaired by common workmen; 7°, are cheap".

14a. "The Ropes defining the Run must be strained at lowest possible Level".

21. "A moderate DEVIATION from the true Float-Course is admissible", .. (13).

"The admissible Deviation is greatest near mid-channel, decreases slowly towards the banks, and rapidly close to the banks", ... (14).

"Strict accuracy in the position of the Pendants is not essential", .. (15).

23. "To save time the Dead Run must be reduced to a minimum near the banks, especially with Surface-Floats", ... (19).

"Subsurface Floats require increased length of Dead Run as the depth of submergence increases", ... (20).

26. "For accurate timing the eye should be free to watch the visible phenomena, whilst the timing should be done wholly by ear", ... (21).

"Similar observations at beginning and end of the timing should be done by the same Observer, so as to eliminate his personal equation", ... (22).

27. "Precision in timing admits of Short Runs which both save time and conduce to accuracy of Course", ... (25).

27a. "Long RUNS waste time", ... (26).

"The Run should be the shortest compatible with good timing", .. (27).

29. "With really good timing a 50′ Run is a good STANDARD for general use; smaller Runs must be used near the banks", ... (29).

31. "No Float which is not a 'Good Float' (as below) should be recorded", (30).

31a. "The sole criteria of a GOOD FLOAT, *i.e.*, of a Float's passage being worth record, are that it should run both 'free' and in 'fair course'".

CHAP. V.—DETAILS.

5. "The mean of highest and lowest free water-levels within a short interval may be accepted as the Average Free Water-level at the time", ... (4).

8. "This Free Water-level is usually slightly *higher* than the Still Water-Level", (5).

"For great accuracy (*e.g.*, for Surface Slope-measurement) the Free Water-level should be taken", ...(6a).

"For ordinary work either the Free or Still Water-level may be used; the same one should be used constantly at same Spot", ...(6b).

11. "The Average Free Water-level on either bank may be accepted (except in a high cross-wind) as the Free-Water level of the Site", ...(9a).

"But in a high cross-wind the mean of Results on both banks should be taken", ... (9c).

12. "The Mean of the initial and final water-level of an Experiment may be accepted as the Mean Water-level of the Experiment", ...(10a).

13. "In Rough Beds Average Cross-Sections should be determined from Average Soundings, *i.e.*, the Average of several Soundings along a Sounding Course".

"Average depths determined from these should alone be used in computation".

15. "The determination of Average Depths depends ultimately on the water-level determination; so that the Depths are liable to an error of a few hundreths of a foot if the Water-level be taken on one bank only in a high cross-wind", (11).

[This of course affects all computed Results (*e. g.*, Discharges past a vertical, Cross-Section Areas, and Cubic Discharges) into which the Depth enters as a factor].

17. "Soundings should be taken whenever possible (*i. e.*, in depths not > about 15′) with a Sounding Rod (not with a Sounding Line) from a Boat floating freely down-stream".

CHAP. VI.—UNSTEADY MOTION.

4. "The velocity at any one point of water in motion is very variable both in magnitude and direction, and the variation is very rapid, *i.e.*, the Motion is essentially Unsteady", ... (4)

4a. "Single velocity-measurements are of very little practical use", ..(7*a*).

5. "Average velocities are the only intercomparable Results at different points", ... (9).

"Average velocities are the only Results of much practical use", ...(10).

6. "Hydraulic Experiments on large bodies of water must necessarily be both *tedious* and *expensive* from the tediousness of determining Average velocities",(11).

7. "The Unsteady Motion necessitates the use of a large stock of Floats".

8 & 12. "When velocity-measurements at numerous points are required, the External Conditions will probably change in the time required for obtaining so many Average values. To meet this difficulty, the work should be done in SETS of a few (say 3) velocity-measurements only, which should be done as rapidly as possible at each point in turn".

8 & 13. "The details of each Set will be under nearly same External Conditions: and the Average Results of many such Sets form a SERIES under nearly same Average External Conditions".

10. "The Mean of about 50 distinct velocity-measurements (done in rapid succession) may be accepted as an AVERAGE VELOCITY-MEASUREMENT", ..(12).

13 & 14. "Only such Sets as are under tolerably similar External Conditions should be combined into SERIES. The combination should be such as to eliminate personal equation".

15. "The Average Velocity-Curves for Sites of regular contour in a long uniform straight Reach are pretty regular Curves, generally convex down-stream", (13).

"The departures from regularity in actual Diagrams are probably due to—
1°, insufficient number of velocity-measurements to yield good Averages,
2°, irregularity of contour of bed and banks at and near the Site".

16. "Discharge-measurements and Mean Velocity-measurements taken from single SETS are only FAIR AVERAGE values".

17. "The stream-lines of water in motion interlace freely in all directions",(16).

18. "There is Average Steady Motion", ...(17).

19. "The Unsteady Motion of water is analogous to that of the wind".

20. "The property of Unsteady Motion enormously increases the difficulty of forming a rational Theory of Fluid Motion".

CHAP. VII.—SURFACE-SLOPE.

2b. "Surface-Slope measurement is an extremely delicate operation".

2a,b. "The Slope-Length should be the shortest yielding a Surface-Fall greatly exceeding the ordinary oscillations of Free Level", ... (4), (5).

2b. "The Water-Levels at the two Slope-Points should be taken *simultaneously*", ... (6).

"The Slope-Points should be equidistant from the Centre Section of the Experimental Site, in positions free from eddies and back-waters, where the motion of the water is as quiet as possible, and with nearly equal surface-velocities past them: the channel also should be *symmetrical*—both geometrically and physically—about the Site throughout the Slope-Length and for some distance above and below", ... (7)—(8*a*).

5a. "Different Slope-Lengths give different Results, so that it is probably impossible to obtain true Local Surface-Slope measurements", ...(10).

"The same Slope-Length should always be used at any one Site", ..(11).

6a. "The Surface-Slopes of opposite banks are not generally equal", ..(13).

"Local Surface-Slopes should be deduced from simultaneous measurements on both banks", ... (14), (15).

15b. "The Local Surface-Slope does not change in any obviously regular manner with change of depth", ... (29*a*).

"It partakes of the changes of the Surface-Falls of the Upper and Lower Sub-Reaches", ... (29*b*).

"It decreases rapidly with increase of Obstruction at Tail", .. (29*c*).

16e. "At times of High Water-level with no temporary Obstruction at Tail the Free-Surface sinks as follows:—

in nearly parallel lines in the Upper Sub-Reach, ... (30*a*).

in converging lines (with gradually flattening gradient) in the Lower Sub-Reach", ... (30*b*).

"Obstruction at the Tail flattens the Free Surface-Gradient for a long distance back; with greatest effect in the region below the level of the Crest of the Obstruction, and with rapidly decreasing effect beyond this", ... (30*c*,*d*).

17. "The Surface-Gradient is chiefly determined by the Control (especially Obstruction) at the Tail", ... (31*a*).

CHAP. VIII.—SURFACE-CONVEXITY.

8b. "The measurement of convexity or concavity of (*i. e.*, across the) Free Surface is an exceedingly delicate operation".

5. "The water-surface in a long straight Reach with pretty straight banks is—on the average—nearly level across", ... (5).

CHAP. IX.—SUBSURFACE VELOCITY INSTRUMENTS.

2b. "Twin Floats do not—in consequence of the Unsteady Motion—give a proper value of the velocity in the path of the Sub-Float", ... (3).

4. About the "ESSENTIALS of a Double-Float".

8—8. viii. Detail of "OBJECTIONS to the Double-Float".

8, iii & vi. "The most serious Fault of the Double-Float is the rapid increase of Connector-Resistance with increased depth of immersion of the Sub-Float".

9. "Other Faults can be removed by suitably proportioning the parts of the Instrument".

"The efficiency of a *given* Double-Float decreases with increase of depth of immersion of Sub-Float, and there is a limit of depth beyond which it fails", (5), (6).

"To secure equal efficiency at all depths, the Sub-Float should be increased both in size and nett weight as the depth increases", ... (7).

9a. "The subsurface velocity-measurement made with the Double-Float is attributed to a depth always > the real depth of immersion of the Sub-Float", .. (8).

"and is always more or less affected by the Surface-Float and Connector Resistances", ... (8).

CHAP. X.—VERTICAL VELOCITY-CURVES.

4. "Frequent re-adjustment of the Connectors of Double-Floats is inconvenient". "Subsurface velocity-measurement with Double-Floats can therefore only be done with convenience at fixed depths".

8. "The PROPERTIES of the Average Vertical Velocity-Curves are as follows:—

"The Curves are generally convex down-stream, (except near an irregular bank)", ... (1).

"The maximum velocity is usually below the surface", ... (2).

"The max. velocity sinks (in a rectangular channel) from the centre towards the banks, and is at about mid-depth near the banks", ... (3).

"The velocities near the bed are generally the least", ... (5).

"The mid-depth velocities are generally greater than the means", .. (6).

"The Curves are decidedly flat", ... (8).

"The flatness decreases in a rectangular channel from the centre towards the banks", ... (9).

11. "Vertical Velocity-Curves obtained with the Double-Float are distorted (by Instrumental defects) as follows":—

(*a*), "*Max. velocy. at Surface.* The partial errors are cumulative, the Observation-Curve lies wholly without the true Curve, and the Error increases with the depth", ...(15*a*).

(*b*), "*Max. velocy. below Surface.* From the surface to the max. velocy.-line the partial errors are cumulative; from the max. velocy.-line to a depth where the velocity is equal to the surf.-velocy. they are partly compensatory; below this line they are again cumulative. From the Surface downwards the Observation-Curve lies within the True Curve, crossing it at a point somewhere in the second region above named; below this point it lies wholly without the True Curve, and the Error increases with the depth", ... (15*b*).

11. "The Observation-Curves obtained by use of the Double-Float are all too flat, especially near the bed where the velocities are all exaggerated", .. (16*a*).

12a. "The mid-depth velocity (on any vertical) is subject to great and rapid variation from instant to instant", ... (19).

12b. "but to a less extent than the Surface- and Bed-velocities", .. (20).

13a. "The bed-velocity is subject to much irregularity".

15. "Any marked change of any one velocity is accompanied on the whole by a *similar change* of all the velocities past the same vertical". .. .. (25*a*).

CHAP. XI.—VERTICAL VELOCITY-CURVE FIGURE.

1. "The investigation of the figure of the Vertical Velocity-Curve is a very

delicate inquiry, as the data available (the velocity-measurements) are not good data for the purpose".

3c,d. "The Method of trial and error is altogether unsatisfactory for the purpose if the depth of max. velocity-line, and parameter of the curve are to be discussed".

3e, 6, & 10. "The 'Method of Least Squares' is alone satisfactory in this case".

7. "The Average Vertical Velocity-Curve approximates in general closely to a parabola with horizontal axis",.. .. .. (17).

7b. "The data do not admit of the determination of the depth of max. velocity line and parameter of the curve (which define the position and size of the curve) with any closeness", ... (20).

9a. "The max. velocy.-line is usually above mid-depth on all verticals, and above ¼-depth on verticals at or near mid-channel", ... (24*a*,*b*).

10. "The tracing of the dependence of the quantities Z, p on the External Conditions is very uncertain".

10—11b. "The Mississippi and Bazin Experts. formulæ for the parameter are not based on good evidence".

CHAP. XII.—DEPRESSION OF MAXIMUM VELOCITY.

3a,c. "The position of the line of maximum velocity on any vertical does not depend sensibly on the depth of water, nor yet on the state of the wind", (1) & (3).

4. "Wind must be long continued to sensibly affect the position of the max. velocity-line".

7. "The Air-surface is a part of the Wet Border causing a slight but sensible resistance to the flow", ... (4).

"The depression of the maximum velocity-line is due largely to the Air-resistance", ... (5).

CHAP. XIII.—DISCHARGE PAST A VERTICAL.

2d. "The Arithmetic Mean and Trapezoidal Rules for Areas both err in defect on the whole",.. .. .. (18).

4b. "The Discharge past a Vertical found by use of the Double-Float > the true Discharge when the max. velocity-line is at or near the surface, and approaches equality with it as that line sinks to a depth of about ⅓ full depth. As this line sinks further the Discharge-measurement falls short of the true Discharge, and the Discrepancy increases as that line sinks",.. .. .. (22).

CHAP. XIV.—MEAN VELOCITY PAST A VERTICAL.

2. "The Arithmetic mean of velocities past a vertical is < the Mean Velocity", (2).

3. "The Mean Velocity past a vertical varies less than most of the velocities of which it is the mean", ... (3).

4. "but it is by no means constant", ... (4).

6b. "The Mean Velocity-measurement (U′) exceeds or falls short of the true Mean Velocity (U) past the vertical, according as it is less or greater than the Surface-velocity (v_0)", ... (9).

9b. "For finding Mean Velocity past a vertical by velocity-measurements *at more than one point*, the formula $u_m = \frac{1}{4}(v_0 + 3v_{\frac{2}{3}H})$ combines accuracy with the greatest practical convenience", ...(33*a*).

9d. "The Mean Velocity-line is always below mid-depth",.. .. .. (36).

9d,e. "The Mean Velocity past a vertical can only be approximated to by velocity-measurement *at a single point*", ... (37*b*).

9f. "The Average Mid-depth Velocity is generally > the Mean of its vertical".(46*a*).

10a. "The ratio of Mean to Mid-depth velocity is *not constant*, but liable to about 16 per cent. variation", ... (50).

11a. "The Mean Velocity past a central vertical increases and decreases on the whole with increase and decrease of either Depth or Surface-Gradient", (51), (52).

11b. "The relation of the Mean Velocity to the External Conditions is too complex to be worth endeavouring to trace out by mere Experiment, *i.e.*, without some guide from a rational Theory".

12. "A much closer approximation to the Central Mean Velocity may be obtained by direct velocity-measurement of any one primary velocity past centre vertical (*e.g.*, the surface, mid-depth, &c.) than from any known formula in terms of Surface-Gradient", ... (53).

13. "The best practical mode of Mean Velocity-measurement in depths over 15′ is to attempt only approximation by velocity-measurements at $\frac{2}{3}$-depth as a general rule, or at $\frac{1}{10}$-depth close to vertical banks".

CHAP. XV.—RODS.

4. About the "ESSENTIALS of a good Rod".

5. "The Full Length of a Rod should only slightly exceed its immersed Length", ... (2*a*).

"A Long Rod is quite unfit for use with small immersion", ... (2*b*).

7f. "Top-hooks are a useless addition to a Rod".

8e. "The Rod-velocity of a Rod nearly grazing the bed gives an approximation to the Mean Velocity past the vertical generally closer than that given by the Double-Float", ... (16).

9. "Rods move more steadily than any other sort of Float".

10. "The Advantages of Rods (for measurement of Mean Velocity past a Vertical) are, as compared with the Double-Float, as follows :—

"They are free from the uncertainty attending the Instability and unknown Lift of the Double-Float", ... (18).

"They give an approximation to Mean Velocity past a vertical usually closer than that given by the Double-Float", ... (19).

"They give the Result more rapidly", ... (20).

"They are more easily handled, and less delicate", ... (21).

"They are simpler in construction, cheaper and more durable", ... (22).

10. "For measurement of Mean Velocity past a Vertical, the Rods should supersede all other Instruments in cases favorable to their use", ... (23).

11. "The Conditions favorable to the use of Rods are :—

"A Reach of nearly uniform cross-section and average bed-slope throughout a great length", ... (24).

"The Bed should be tolerably even lengthways at and near the Site", .. (25).

"The Depth should not exceed about 15′ at and near the Site," .. .. (26).

12. "The Site should be prepared for use of Rods as follows :—

"The Bed should be dressed to a tolerably uniform cross-section and longitudinal slope for a length of say 250′", ...(29a).

"The Banks should be dressed to a tolerably uniform slope for a length of say 250′; and, if likely to suffer erosion, should be revetted with masonry," (29b).

CHAP. XVI.—THEORY OF ROD-MOTION.

10. "The Rod-velocity line is—within the limits of practice—always *somewhat more deeply seated* than the line of Mean Velocity past its immersed length",(18b).

"and is therefore—within the limits of practice—always *somewhat less than* the Mean Velocity past its immersed Length", ...(18c).

"For mere accuracy of measurement of Mean Velocity past a vertical, the immersed Length of a Rod should be *decidedly less* than the full depth on the vertical", ... (19).

11. "The proper (immersed) Length of Rod is from ·950 to ·927, or on an average ·94, of the Full Depth of water", ... (22a,b).

CHAP. XVII.—TRANSVERSE VELOCITY-CURVES.

5. "For tracing the figure of the Transverse Velocity-Curves the Float-Course Spacing in a Canal should be—

1°—"symmetric about mid-channel".

2°—"wide spaced over the level part of the bed".

3°—"closer spaced with approach to the banks, and with one Float-Course at the foot of each bank".

4°—"closest spaced nearest the edge".

5a. "If Discharge-measurement is the aim in view, there should be a primary division into Spaces as above, and the Spaces should be sub-divided into a number of Sub-spaces which should be multiples of 2, 3, or 6".

12. "The Properties of the Average Transverse Velocity-Curves are as follows :—

12, i. "The velocity-variation (in any one curve) approximates to the following distribution (in the case of a symmetric cross-section with a level or wholly concave bed, in a long uniform straight Reach), ... (3).

"the maximum velocity near the centre", ... (3a).

"a very slow decrease of velocity from the centre towards both banks, which becomes more rapid with approach to the banks, and is very rapid close to the banks",.. .. .. (3b—d).

"the curve is wholly convex down-stream", ... (3e).

"and is symmetric about mid-channel", ... (3f).

12, ii. "Every marked change in the figure of the bed produces in general a marked effect on the figure of the velocity-curve", as follows :— .. .. (4).

"Increase of depth tends to increase of velocity, and *vice versâ*", .. (4a).

"The maximum velocity-line tends to be in the deepest channel (if sufficiently far removed from the banks)", ... (4b).

"A convexity in the bed causes a concavity in the velocity-curve and *vice versâ*",(4c).

"These effects are more marked in shallow than in deep water", ... (4d).

12, iii. "Velocity at same point of Like Curves increases and decreases *cæteris paribus* with rise and fall of water-level", ... (5).

12, iv. "Like Curves are similar under similar External Conditions", .. (6).

12, v. "Like Curves of equal mean velocity are *cæteris paribus* equally flat as a whole", ... (7).

12, vi. "Curves of low velocity are *cæteris paribus* flatter than those of like kind of high velocity", ... (8).

12, vii. "The Flatness of a Curve does not depend so much on the general *depth* of water as on the Mean Velocity, so that Curves at low water are not necessarily flatter than Curves (of like kind at high water)", ... (9).

12, viii. "These Curves are sharply rounded over sloping or stepped banks, and more fully rounded near vertical banks", ... (10—10*c*).

12, ix. "At Sites of similar character Like Curves are—each taken as a whole—flatter throughout at the wider Sites", ... (11).

12, x. "Of Unlike Curves under similar External Conditions in the same rectangular channel, the Mid-depth Curve is usually the outer, (except near the centre,) the Mean Velocity-Curve intermediate, and the Bed Curve the inner", ...(12*a*).

also, "the Mean Velocity-Curve is one of the flattest, and the Surface Curve the most fully rounded", ...(12*b*).

14. "The forward velocity near the edge decreases rapidly with approach to the edge", ... (13).

14b. "At the edge the forward surface-velocity is very small, (perhaps zero)",(13*b*).

"Near the edge there is a persistent flow (at and near the surface) from the edge towards the centre, most intense nearest the edge, and decreasing rapidly with distance from the edge", ... (13*c*,*d*).

CHAP. XVIII.—TRANSVERSE CURVES—GEOMETRIC FIGURE.

1. "The investigation of the figure of the Transverse Velocity-Curves is a very delicate inquiry, as the data available (the velocity-measurements) are not well suited to the purpose".

2. "In Curves of like kind with same water-level at same Site, the velocities at same points are nearly proportional, so that such Curves are approximately parallel projections of one another", ... (1) & (1*a*).

3—4. "The Transverse Curves resemble semi-ellipses in general shape, but with flatness increasing as the water-level sinks in such a way that the exponent (*n*) (of the order of ellipse) increases as the water-level sinks".

6. "The Wet Border (including the Air in this term) is the ultimate source of retardation of flow",.. .. .. (11).

"Velocity at any point is probably a function of the 'average effective distance' from the Wet Border".

CHAP. XIX.—AREAS AND DISCHARGES.

11. "The number of repetitions of any Observation-measurement should be proportional to its 'weight' in the computation formulæ", ... (13).

13. "The Trapezoidal and Arithmetic Mean Rules err in defect in the long run: and when the bed is concave the Error is not necessarily very small".

15a. "The chief advantage of the process of Discharge-measurement used in this Work is the completion of the Field-work of a single Result within a moderate time within which the External Conditions (except Wind) may be *presumed to have been tolerably constant*".

17a. "The Cubic Discharge increases and decreases rapidly with rise and fall of water-level", ... (14).

"but depends to at least an equal extent on velocity".

18. "The Cubic Discharge is sensibly constant from instant to instant",.. (15).

19c. "A cross-wind is liable to cause excess or defect in Cubic Discharge-measurement according as it blows to or from the Gauge", ... (18).

19d. "The chief source of variability of successive Cubic Discharge-measurements (under apparently the same External Conditions) is that—in consequence of the Unsteady Motion—each single Result is an imperfect one".

21. "Discharge-Tables should be Tables *of double entry*, showing both Gauge-Reading and Surface-Slope or -Gradient as Argument".

CHAP. XX.—MEAN VELOCITY.

2. "The Arithmetic Mean of velocities errs in defect".

3. "The Mean Velocity past a Transversal, and the Mean Sectional Velocity are less variable from instant to instant than most of the individual velocities", .. (3).

3a. "The Mean Velocity past a Transversal varies sensibly from instant to instant", ... (4).

3b. "The Mean Sectional Velocity is constant from instant to instant, and in a higher degree than the Cubic Discharge", ... (5 & 6).

5. "The chief source of variability of successive Mean Velocity-measurements (under apparently similar External Conditions) is that—in consequence of the Unsteady Motion—each single Result is an imperfect one".

14. "There is a *general sort of agreement* in the variation of the Mean Surface and Central Surface Velocities (U_o, v_o), and also in that of the Mean Sectional, Central Mean, and Central Surface Velocities (V, U_o, v_o); also of the quantity $\sqrt{RS}$ with the latter three", ... (14).

15. "The Mean Surface and Central Surface Velocities (U_o, v_o), and also the Mean Sectional, Central Mean, and Central Surface Velocities (V, U_o, v_o), and the quantity $\sqrt{RS}$ increase and decrease with increase and decrease of either Hydraulic Mean Depth or Surface-Gradient", ... (15)—(18).

16c. "Surface Velocity-measurements,—and therefore also Discharge-measurements depending on them,—are liable to be under- or over-estimated in high up-stream or down-stream wind", ... (19 & 22).

"The Mean Velocity-measurement is only slightly—if at all—affected by up- and down-stream wind", ... (20).

"Surface Velocity-measurements made in high up- or down-stream wind are quite unsuitable data for Discharge-computation", ... (23).

16d. "The Mean Velocity-measurement is not sensibly affected by a high cross-wind, but is *attributed* to an abnormal Gauge-Reading", ... (24).

17. "The ratio $c = V \div U_0$ increases in general with increase of depth, and probably also with decrease of velocity or surface-slope", ... (25 & 26).

18. "The variation of the ratio $c = V \div v_0$ is very obscure, (probably in consequence of the disturbing effect of Wind on the surface velocity".

19. "The ratio $C = V \div 100\sqrt{RS}$ increases and decreases generally with increase and decrease of hydraulic mean depth", ... (27).

19a. "and also depends in some complex manner on the Surface-Slope",.. . (28).

19b. "and also on the nature of the banks and bed of the Site",.. .. (29).

21. "The form of the Bazin Co-efficient $C_b = \left(\alpha + \frac{\beta}{R}\right)^{-\frac{1}{2}}$ is defective",.. (36).

22a. "Mean Velocity- and Cubic Discharge-Measurements obtained by applying Bazin's Co-efficient c_b for reducing Central Surface Velocity-Measurements to Mean Velocity are usually *under-estimated*", ... (40).

22b. "The under-estimation is so great that this Co-efficient is of little practical use in Earthen Channels",.. .. .. (41).

22c. Bazin's relation $c_b = 100\,C \div (100\,C + 25{\cdot}34)$ is fundamentally incorrect as a relation between $c = V \div v_0$ and C", ... (42).

23c. "Kutter's Co-efficient (C_k) is one of pretty general applicability",.. (45).

"When the Surface Slope-measurement is a *good average*, it gives Results whose Error will probably seldom exceed 7½ per °/₀ in Large Canals", ... (46).

24. "The Mississippi Experts. Formula is useless as a *general expression* for Mean Velocity, (except perhaps in cases of very low surface-slope)", .. (52).

26. "Further experimental research for an improved Mean Velocity Formula is an almost hopeless work until the *proper* functional form is suggested by an improved rational Theory".

27a. "A close approximation to Mean Velocity is more likely to be obtained by use of formulæ depending on Velocity-measurement than on Surface Slope-measurement", ... (55a).

27b. "Central Mean is to be preferred to Central Surface Velocity-measurement for use in approximating to Mean Velocity", ... (57).

28. "The connexion between Mean Velocity and any other primary Velocity is of a more intimate and simpler kind than between Mean Velocity and Surface-Slope; the former being probably merely a geometrical, whilst the latter is a physical, relation".

29. "For rapid approximation to Mean Velocity a *good* Average Central Mean Velocity-measurement is (at present) the most reliable", ... (58).

CHAP. XXI.—DISCHARGE-VERIFICATION.

4. "For comparison of successive Cubic Discharge-Measurements *at the same Site*, such as are done in immediate succession are much the most suitable".

9c. "For comparison of the Cubic Discharges *through successive Sites*, the Field-work should be either *simultaneous* or else *in same* body of water at all the Sites".

12. "Under favorable circumstances the process of Cubic Discharge-measurement used on this Work yields consistent Results", ... (6a).

12. "The Discordance between successive comparable Results may be expected to be seldom over 3 per cent., (when the Conditions are nearly similar, and the circumstances favorable)", ... (6b)—(7c)

CHAP. XXIII.—CURRENT-METER WORK.

3—3, xiv. Detail of "Objections to Current-Meters".

6. "By use of a proper Current-Meter Lift the following advantages are secured, viz., Certainty of Orientation and Position and of Gearing and Ungearing the Current-Meter, also Measurement of forward velocity (besides some minor advantages)".

10. "By separation of the "recording works" and connecting them electrically with the Fan, the following advantages are secured, viz., Reduction of disturbance of the water, Certainty of gearing and ungearing, Increased delicacy, Saving of delay of lifting to read, and Vision of the motion of an Index copying the movement of the Fan".

11. "The uncertainties attending these Instruments as at present made are very great".

CHAP. XXIV.—SILT.

8c. "There is no obvious connexion between the Velocity and Silt-Density *at different parts* of a Site", ... (6).

"The Silt-Density varies from instant to instant at one and the same point".

9c. "The Silt-Density, and Silt-Discharge do not appear to depend sensibly on either the depth or velocity at a Site", ... (7).

"The Silt-Density and Silt-Discharge in the Ganges Canal depend chiefly on the quantity of Silt present in the Supply admitted into the Canal", ... (8).

CHAP. XXV.—EVAPORATION.

8b. "The Evaporation from (an Evapometer floating on) a large still water-surface or on a river is much less than from a small vessel on dry land, (from the liability of the latter to become superheated)".

8d. "The Evaporation from the Ganges Canal near Roorkee averages about $\frac{1}{10}''$ daily (out of the rainy season)", ... (1).

10. "The Evaporation-Loss is about $\frac{1}{180}$ part of the Full Supply of the Canal (or about 10 minutes' Full Supply daily"), ... (2) & (2*a*).

CHAPTER XXVII.

COST.

***Preface.*—The general Reader should omit the details in the Table of Art. 1.**

1. Cost.—The Amounts sanctioned yearly by Government, and the *actual* Expenditure of each financial year (up to April 1881) on the whole Work are shown in the Table below, with the Expenditure separated for ready reference under the three Heads of Field-work, Reduction, and Publication (denoted by F, R, P respectively in last Column).

Application.		Government Sanction.			Sanction.	Expenditure.				
No.	Date.	Govt.	No.	Date.	Rupees.	Year.	R.	A.	P.	Charge.
459	of 13-5-'74	Indian Govt. Orders.	480	of 27-7-'74	1,750	1874-75	824	4	0	F
						1875-76	525	14	3	F
1268	„ 6-4-'75		359I	„ 1-10-'75	3,000	1875-76	1,526	12	3	F
157	„ 15-1-'76		129I	„ 16-3-'76	5,500	1876-77	5,478	10	10	F
4849	„ 24-11-'76		92AI	„ 22-3-'77	6,000	1877-78	7,156	7	8	F
1411	„ 28-4-'77		389	„ 22-1-'78	1,650	„ „				F
328H	„ 15-10-'78	N. W. P. Govt. Orders.	C949W	„ 25-10-'78	1,777	1878-79	*1,026	14	6	F
Various.			133E	of 5-5-'79	17,307	1878-79	6,700	12	4	F
683H	of 12-8-'80		3474E	„ 25-11-'80	3,044-8	1879-80	1,425	15	7	F
707H	„ 6-12-'80		3943E	„ 21-12-'80	461-6	1879-80	6,187	8	11	R
						1880-81	3,244	9	6	R
725H	„ 24-1-'81		996E	„ 8-3-'81	155-2	1880-81	4,471	0	0	P
						Grand Total Expenditure, ..	38,568	13	10	
						Deduct Realization by Sale of Stock, ..	1,397	6	4	
						Nett Total Expenditure, ..	37,171	7	6	

The details above are printed here chiefly for sake of permanent record and official reference. The Totals alone—shown in Abstract below—will probably be of any general interest :—

* This was for Tools and Plant, most of which were transferred (after 4 months' use) to the Northern Divn., Ganges Canal, *without charge*, so that only a part of this item is fairly chargeable to the Experiments.

Abstract of Expenditure.	RS.	A.	P.
Total Expenditure on Field-work, ...	24,665	11	5
" " " Reduction, ...	9,432	2	5
" " " Publication, ...	*4,471	0	0
Grand Total Expenditure, ...	38,568	13	10
Deduct Realization by Sale of Stock, ...	1,897	6	4
Nett Total Expenditure, ...	37,171	7	6

These Totals show the ACTUAL NETT EXPENDITURE only to end of the financial year 1880-81. They are therefore not quite final. There will be some small expenditure in distribution of the Work falling in the financial year 1881-82, and there may be some modification* of the charge for Publication when the Work is issued.

2. **Total Cost.**—The Total Expenditure above shown (Rs. 37,171-7-6) is only the *direct expenditure* which appears charged to the Experiments in the Public Accounts. Many charges which might fairly have been charged to the Experiments were borne by other Departments, thus :—

1°. Many of the Observers were *lent* by the Irrigation Department, *free of charge*, viz. :—
   Sergt. Reynolds from 21-2-'78 to April '79,
   Mr. Andrews, Sergt. Bell, Mr. Callaghan, Mr. Clowsley, and Mr. Smith for the periods stated in Ch. II, 4.

2°. The services of the Superintendent were set free by the Thomason College, wholly from Novr. '78 to April '79, and in part from May '79 to Octr. '80, *without charge* to the Experiments, beyond a comparatively small "officiating allowance" to the officers who took up his College duties, (amounting in all to Rs. 1,670-15-6.)

3°. The Thomason C. E. College provided *without charge*—
   (*a*). Office accommodation and furniture throughout the whole period.
   (*b*). Use of Surveying and other Instruments " " " "
   (*c*). Use of a large Camp Equipage from January to March 1879.
   (*d*). Stationery throughout the whole period.

4°. Several Departments lent various expensive Plant for long periods, *free of charge*—
   (*a*). 3 iron boats and 1 Pontoon Raft were lent by the Irrigation Dept.
   (*b*). 1 Pontoon Raft was lent by the Bengal Sappers and Miners.
   (*c*). 3 chronometers were lent by the Survey Department.

5°. A certain amount of "preparation of Sites" was undertaken by the Irrigation Department *free of charge*.

It is difficult now to estimate what would have been the charges under these several heads : they may be set down roughly as follows :—

* This is *only the Estimated Cost* of Publication of the (200) copies taken by Government for distribution: the Actual Cost—when the Work is issued—may differ a little from this.

No. 1°, Rs. 5,500; No. 2, Rs. 6,000.

Nos. 3°, 4°, 5° are impossible to estimate properly now, say Rs. 5,000.

This would swell the Virtual Total Cost of the Experiments to about Rs. 54,000.

This may seem a large sum for the sort of work, and for the practical Results obtained; but this class of work is (from causes explained in the Chapter on Unsteady Motion, Ch. VI, 6) *necessarily very expensive.*

Nearly the whole expense of such work is the skilled Establishment: the cost of Stores is comparatively small. The heaviest item in these Experiments is actually the salaries of the Observer Staff, (European Overseers drawing from Rs. 185 to Rs. 100 a month.)

[Skilled superintendence would—as a rule, in India—form by far the heaviest item. In these Experiments however it was a *very small item*; for the first two years the Superintendent gave his services free; from April 1877 till December 1880 Government granted a "superintendence allowance"* of Rs. 150 a month].

END OF PART IV.

* A special Superintendent would have cost from Rs. 350 to (say) Rs. 1,000 a month according to his experience and standing in the Service.